REPORT RESUMES

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REINFORCEMENT, A REVIEW OF SELECTED RESEARCH. FINAL REPORT, PART 1.
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A GENERAL SURVEY OF LITERATURE IN THE AREA OF REINFORCEMENT WAS MADE DURING THIS STUDY. THE REPORT SUMMARIZED RESEARCH WHICH HAS BEEN CONDUCTED BY VARIOUS AUTHORITIES IN THE FIELD. THE SUBJECT OF REINFORCEMENT WAS DIVIDED INTO--(1) THE PROBLEM OF DEFINING REINFORCEMENT, (2) POSITIVE REINFORCING STIMULI, CONSUMABLES, (3) POSITIVE REINFORCING STIMULI, EXTEROCEPTIVE STIMULI, (4) POSITIVE REINFORCING STIMULI, SOCIAL STIMULI, (5) AVERSIVE STIMULI AS REINFORCERS, (6) DRIVE LEVEL IN RELATION TO REINFORCEMENT, (7) TIME CONDITIONS RELATED TO REINFORCEMENT, (8) CHARACTERISTICS OF THE REINFORCING EVENT, AND (9) FEEDBACK AND KNOWLEDGE OF RESULTS AS REINFORCEMENT CONCEPTS. IT WAS FOUND THAT MUCH OF THE RESEARCH LITERATURE WAS OBTAINED FROM THE STUDY OF VARIOUS STRAINS OF LABORATORY RATS. THE COMPILERS OF THIS SURVEY HAVE STRESSED CAUTION IN GENERALIZING ANY OF THE FINDINGS WITH PARALLELS IN HUMAN BEHAVIOR, INASMUCH AS MOST OF THE RESEARCH HAS BEEN REMOTE FROM THE PROBLEMS OF ORGANIZING LEARNING IN SCHOOLS. (WB)

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REINFORCEMENT

A Review of Selected Research

by

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Preface

The concept of reinforcement has long been seen by psychologists connected with education as a tantilizingly simple and elegant model of all that transpires in a classroom. The authors of this monograph were among those that were sufficiently intrigued with attempts to apply reinforcement concepts to the planning of classroom learning that they had become engaged in a program of empirical research in the area. As work progressed it became apparent that a thorough review of research involving the concept of reinforcement would have to be undertaken in order to clarify our own thinking concerning the concept and in order to become more familiar with the basis from which current applications of reinforcement learning concepts to educational problems have evolved. A superficial inspection of the literature showed that the term *reinforcement* is used with a multitude of meanings, often without a writer indicating the precise meaning to which he subscribes. The semantic problem which this poses added enormously to the task of reviewing available knowledge.

A cursory review of this monograph shows that a large fraction of the research literature which surrounds the topic is derived from the study of various strains of laboratory rats. This fact, by itself, should lead to caution in generalizing any of the findings to human behavior. While research related to the concept of reinforcement has resulted in the discovery of means of controlling the behavior of a few subhuman species, the evidence that such results can be applied to the control of human behavior is at best flimsy and often absent. While this is not the only area in which psychologists have shown a flamboyant and colorful flair for overgeneralizing their results to the solution of educational problems, it is

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nevertheless a notorious one. It is hoped that this monograph will draw to the attention of psychologists in education to the remoteness of most research on reinforcement to problems of organizing learning in schools. The literature does suggest interesting ways of formulating educational problems for further research, but the authors are sceptical about using present knowledge as a basis for widespread educational reforms.

Assistance in the completion of the manuscript has come in one form or another from many sources. The authors are indebted to the Cooperative Research Branch, U. S. Office of Education, for the financial support afforded through Contract number 2-10-010, to Dean Asahel D. Woodruff who has provided a nurturant environment, and to Madge Jackson Nickell for her assistance in the preparation of the early drafts of the manuscript.

The hiatus between the rough drafts and the final product was bridged with the able assistance of Patty Quinn Taylor, Vivienne Selby, and our more recent addition to the staff, Jeanette J. Jackson. They typed the preliminary drafts and the final manuscript, proof-read and checked the copy, and have brought the manuscript to fruition in spite of the obstacles we have placed before them.

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THE PROBLEM OF DEFINING REINFORCEMENT

A general survey of the literature in the area of reinforcement is quite sufficient to impress one with the importance of the role of language in the development of a science. It is not at all unusual to find strikingly different phenomena traveling under the same name, to find one term referring to a multiplicity of events, either observed or implied. These observations argue persuasively for prior consideration of the use of terms. Such concern, of course, is by no means new. Historically it has led to surprising results. Lavoisier (translated by Robert Kerr 1952) at the end of the eighteenth century observed that his treatise on chemical nomenclature was transforming "itself by degrees without my being able to help it, into a treatise on the Elements of Chemistry" (p. 1). In considering further the role of language in science, he expressed suscinctly the notion that language should serve as a model of reality. "Like three impressions of the same seal, the word ought to produce the idea, and the idea to be a picture of the fact" (p. 1). This idea has a charm about it that cannot be denied. Given the correspondence between words and facts, the relationships between the words both preserve and express the relationships between facts. The language becomes, in a very real sense, a linguistic model of the phenomenal world.

But the model is of necessity imperfect and incomplete. Every object and event is unique in its occurence in its particular time and place; it would be quite impossible to have a one-to-one correspondence between words and things or events. Instead, it is important to develop language so as to preserve the differences that make a difference by employing terms that either stand for classes of objects, events and relationships, or denote attributes that are characteristic of these. To this extent, scientific discourse is admittedly abstract, but hopefully it has its feet on the ground and will be graced with wide applicability.

In addition to the strategy of employing general class concepts, it is often the case that constructs or intervening variables are used for the purpose of introducing order into what might otherwise appear as disorderly phenomena. For these, also, it is important to operationally define the empirical footing upon which the term rests. To the extent that this is not done, reliance is placed upon the connotations of the terms, - the associations we make to the terms. In this case, we are, in effect, defining terms through the use of other terms without reference to the empirical data. This has historically been characterized as the analytic approach. While the approach provides for internal consistency, it is rooted in circularity. The referential characteristics of propositions based on these definitions are, fortuitous at best, and at worst non-existent. The fact that definitions of this type survive stands in mute testimony to the importance of connotation as well as denotation. It further suggests that out-thereness is not sufficient to explain language or linguistic behavior.

On the basis of these considerations, one might fully expect to find that the word reinforcement would be characterized by a conventional definition, a definition of nearly universal acceptability. This, of course, is far from the case. While it is generally contended that a definition, the assignment of a referent to an utterance, symbol or word, gains currency through convention, it seems more likely that convention, the generalized acceptance of a term, is contingent upon currency; we accept a term because we use it rather than use it because we accept.

it. And the conditions of our use are far more complex than a simple dispassionate consideration of the "conventional referent" of the term. This at least in part explains why it is that the "meaning" of a term shifts from one context to another.

The denotation of a term can be undertaken both formally and informally. A formal definition, optimally, is a careful statement of the conditions under which the term is to be used, a designation of the processes or events for which the term is to substitute (and hopefully these will be of an empirical nature). Informally, the term will substitute for other expressions in context, these expressions being statements of the foregoing type, or one noun may simply substitute for another. The literature affords an abundance of both types of definitions for reinforcement.

Little would be accomplished by attempting to legislate at this point how the term reinforcement should be used, but a great deal can be accomplished by surveying the use of the term as it occurs in the literature (conducting an empirical study of the usage of the term). If nothing more, this will alert us to the variety of situations to which the term is typically applied. In addition, such a survey will afford us the opportunity of specifying the definition that we have accepted for the purpose of the current survey of empirical findings.

Definitions of Reinforcement

Wolpe (1950) very adequately communicates the typical connotation of reinforcement when he defines it as the process of establishing functional neural connections. For him a functional neural connection is that which mediates a stimulus-response sequence (excluding those resulting from maturation), and, as Seward (1950) points out, this is coextensive with learning. Wolpe, further defines a reinforcement as "an individual event that has the effect of initiating or strengthening a functional neural connection (1950, p. 20)." It might be pointed out that these definitions come closer than any others to be reviewed to what Tolman (1958) has termed the switchboard position in learning. But as Spence (1950) has pointed out, a knowledge of neural connections, or for that matter speculations concerning neural connections, are not necessary for an empirical study of behavior. Hull (1950) eliminates mention of neural connections and limits the scope of reinforcement to include drive reduction in his postulate on primary reinforcement. He says, in effect, that when a response, a stimulus, and a diminution of receptor activity characteristic of a need are closely associated, there is an increment in the tendency for the stimulus to elicit the response. Many writers have construed from this that any time there is an increased tendency for a stimulus to elicit a response there is implied a diminution of a drive. To this extent Hull's definition is what Verplanck (1957) would term a theoretical term, for the processes or events are not immediately available to experimental confirmation. It is one, incidentally, that has been very productive in generating hypotheses, for Hull offers empirical definitions of both drive and response tendency. Be this as it may, we can fix upon the empirical content of the definition and note that for Hull, reinforcement is the increased tendency for a stimulus to elicit a response which results from the occurrence of some event. This is perfectly compatible with the definition tendered by

Seward (1950), although it necessitates a little change of usage. Seward (1950) says that when a stimulus change following a response that is made in a situation increases the probability that the response will be made in that situation, then the stimulus change is a reinforcer, and the presentation of the stimulus change is a reinforcement. This is essentially the same position taken by Skinner (1938) and Verplanck (1957), except that what Seward has called a reinforcer, they would call a reinforcing stimulus. It so happens, since the scope and the specificity of terms tend to vary inversely, that they also present the most rigorous definitions, and the empirical orientation is one which can provide a basis for research.

Verplanck (1957) defines reinforcement as "the operation of presenting to the animal in operant conditioning, after it has made a response (and therefore contingent on its occurrence), a reinforcing stimulus or of withdrawing a negative reinforcing stimulus." (He gives a parallel definition for reinforcement in classical conditioning.) Verplanck further defines a response as "a class of parts or changes in parts of behavior such that its members can be observed to vary together systematically as a function of time or other environmental variables." If this definition seems labored, the reason rests mainly on the fact that behavior, although generally classed as a noun, is actually an abstraction from a process. We do not see behavior, but rather see a behaving organism. Uniformity in the activity of an organism lends nicely to the classification of some of this activity, and these identifiable sequences are what we mean by response. As Verplanck has suggested, then, response can refer to an identifiable piece of behavior. It is worth noting, however, that the experimenter is making a sizable contribution to the definition.

In Verplanck's definition, a reinforcing stimulus, the presentation of which constitutes reinforcement, is defined as an environmental event contingent upon some response which alters some characteristic of the response on subsequent occasions in conformance with expectations deriving from the empirical concepts of conditioning. It is worth noting that the definition implies a pragmatic test, the change in the response measure being an intrinsic part of the definition as proposed on objective grounds. It would be contradictory to say that there was no change in response in spite of the fact that a reinforcing stimulus was presented. As the definition implies, the animal may be subjected to certain "drive-inducing" operations (specifically, subjected to privation of one sort or another) in order for a stimulus to become a reinforcing stimulus. As Verplanck (1957) notes, food will not be a reinforcing stimulus for a satiated rat. There is nothing in this definition that implies that we must be able to predict when, or under what circumstances a stimulus will be a reinforcing stimulus, but it is evident that this is one of the purposes of the objective science of behavior; only as we are able to distinguish the appropriate stimulus characteristics for every condition of the organism are we able to bring behavior under experimental control.

Verplanck's definition, as it is elaborated by him, is very experimentally oriented. In fact, as it stands, it could not be applied outside of an experimental situation. For that matter, it cannot be applied to an experimental situation until after the reinforcement has occurred; a response change must be demonstrated to show that the stimulus was indeed a reinforcing stimulus. But the definition still leaves latitude for tremendous variability. There is no restriction imposed upon the species used in the experiment, the experimental task (or response definition), and probably of greatest importance, the measure to be used in determining whether or not there has been a change in the response. Typical measures involve an increase in amplitude, speed, frequency, or a reduction in latency, running time, or errors. In addition, time or trials to extinguish or relearn the response has been used as an important measure of the changes produced in a response by learning. Frequently a comparison between two groups on some sort of response measure is used as the criterion of reinforcement. This is important, for there is frequently an interaction between the task and the particular measure of response change used. In some situations the decision that reinforcement has occurred may depend upon the particular measure used. For instance, Lachman (1961) did not find fewer errors for a high drive group than for a low drive group, but found an increase in running speed for rats on a discrimination task with water as the reinforcing stimulus. A stimulus event may be reinforcing for one aspect of a response but not for another.

Emphasis up to this point has been on definitions of reinforcement in which the reinforcing event is a stimulus event, but not all writers on the subject show this preference in the formulation of their definition. For example, Premack (1959) would be inclined to dispense with the concept of a reinforcing stimulus. His position is that certain behaviors have reinforcing properties. According to this conception of reinforcement, a behavior is reinforcing to other behaviors occurring in temporal proximity if it has a higher rate of occurrence than the one which it is to reinforce. Consuming food is a behavior highly reinforcing for bar pressing in the laboratory rat since rats show a high rate of responding to food, but a low rate of responding to bars by pressing them. His thesis is that any response A will reinforce any response B, if and only if, the independent rate of A is greater than that of B. The paradigm fits a wide range of situations, but not all. Since learning experiments typically involve some sort of consumatory response which is made to the reinforcing stimulus, there is no problem in fitting such experiments into the scheme. However, it is especially difficult to define the reinforcing response in many verbal conditioning studies, for this would necessitate defining the response made to such stimuli as "good" or "that's right."

While the conception of reinforcement developed by Premack is ingenious, if not elegant, it leads, as does a stimulus definition of reinforcement, to many difficulties when an attempt is made to evolve a reinforcement mechanism to account for learning phenomena. One of these is that under those conditions to which the term reinforcement applies, the reinforcing response is generally conceived to be a final event in a chain of events and as such does not occur at a strategic time to influence behavior. One way to avoid this difficulty is to postulate that these end responses produce sensory stimulation, both exteroceptive and interoceptive, which, through their action, can have reinforcing properties for temporally related responses.

To this point we have identified the empirical or objective aspect of reinforcement. It is most generally considered to be the experimental operation of presenting a reinforcing stimulus contingent upon a response. This definition is quite in accord with the position taken by Skinner (1938), but we have said nothing about the theoretical aspects of reinforcement. To phrase the issue differently, we have to this point failed to introduce the concept of learning. Of course, not everyone agrees that it is necessary to go further. Skinner (1950) argues very persuasively against it, mainly on the grounds that to this point we have incorporated all of the empirical elements and these are all that are necessary for an objective science. As a matter of fact, he would urge that we dispense with all but one dependent variable, response rate, in most of our experiments. This is most closely akin to the naturalistic approach. It involves a specification of the conditions of observation and a report of the observations. (It seems unlikely that the fact that Skinner's laboratory is particularly suited to this approach has anything to do with his preference. Rather, we suspect the relationship is just the reverse.) But psychologists have a knack for devising laboratory conditions that result in considerable variability of behavior across conditions (unfortunately, within conditions also). It seems reasonable to some that if we had an understanding of the mechanisms of behavior, that is, a good theory of behavior, we would be able to predict the behavior that would occur under different circumstances. In addition, we have not faced squarely the issue that many, in discussing reinforcement, show a propensity for discussing the effects of reinforcement. Reinforcement for them carries the connotation of strengthening something. Hull picks up this element very nicely in his concept of habit-strength and distinguishes between this and performance. Through this concept he would give support to the wide-spread acknowledgment that it does not necessarily follow that you know what you show or that you show what you know. His general model (1943) expresses the response as a function of reaction-evocation potential which in turn is a multiplicative function of "drive" and "habit strength." In his formulation, the effect of reinforcement is habit-strength, and rather than presenting a "drive-reduction theory of reinforcement," he presents a "drive-reduction theory of reinforcing stimuli." In effect, for Hull, reinforcement consists in presenting, contingent upon some response, a drive-reducing stimulus. This is clearly evident in his postulate covering primary reinforcement:

> Whenever an effector activity occurs in temporal contiguity with the afferent impulse, or the preservative trace of such an impulse, resulting from the impact of a stimulus energy upon a receptor, and this conjunction is closely associated in time with the diminution in the receptor discharge characteristic of a need, there will result an increment to the tendency for that stimulus on subsequent occasions to evoke that reaction (1943, p. 80).

The "receptor discharge characteristic of a need" is what Hull means by a "drive," and the needs, in turn, are generally the optimal conditions for biological survival, - air, food, water, etc. The foregoing, when coupled with his postulates covering secondary drives and secondary reinforcement, would theoretically account for all learning phenomena. To this extent, he extends the meaning of reinforcement beyond the experimental setting - indeed the situations to which it will apply are more likely to occur in nature than within the confines of the learning laboratory. But this sort of application is completely antithetical to the objectives Hull had in view in developing his theory. He was completely dedicated to the development of a hypothetical-deductive system that would efficiently predict behavior. To put it differently, he was interested in bringing the variables controlling behavior under experimental control. His theory was not intended as an explanatory principle, but was rather tendered as a hypothesis and the question of whether or not reinforcing stimuli are necessarily related either to primary or secondary drives remains an empirical question.

Hebb (1955) has tendered the hypothesis that the reinforcing properties of a reinforcing stimulus are not necessarily due to a decrement in drive, but rather that they are associated with a return to an optimal drive level. Within this framework, an increase in drive may be the physiological counterpart of a reinforcing stimulus. Hebb has attempted to base his theory on the recent work done on the reticular activating system, and conceptualizes the brain as an active rather than a passive system that simply responds to the stimulus input. On this latter point, he is on rather secure grounds. But once again, it remains a hypothesis, and a fruitful hypothesis only as it lends itself to experimental testing.

An overview of the various definitions of reinforcement leaves one with the impression that some prefer to tie the terms in the definition to stimulus events in an experimental situation while others tie them to response events. In either case, the experimental psychologist defines reinforcement in terms of experimental conditions as they occur in a laboratory setting. This is remote from the popular usage of the term reinforcement with its general, but often vague reference to the everyday affairs of men. It is even a far cry from the laboratory to the school though learning may be studied systematically in both of these situations.

It is important to emphasize here that the definitions considered and those which underlie most experimental work refer to conditions and events in the laboratory and are not intended to have reference to other contexts such as a teaching situation.

Summary

In this chapter, brief cognizance has been taken of the function of language in science, and particular attention has been given to the role of definitions. The definitions of reinforcement included in the survey fall generally into two classes; operational definitions in which the term designates the process of presenting a stimulus condition that results in a measurable change in some response characteristic, and definitions involving theoretical terms such as "habit strength" or "need reduction." Both types of definitions have their particular utility, and the selection of a definition should be guided by the use that is going to be made of it. An empirical definition such as that offered by Verplanck is appropriate for the purpose of an empirical review, for this type of definition refers primarily to observables and finds appropriate use in a review of laboratory research. Such a definition encourages discussion in terms of research results rather than theoretical issues. A definition which avoids theoretical terms also discourages generalization from particular experimental results.

The definition of reinforcement offered by Premack is in a class by itself. In pointing out that a response is reinforced by any other response with a higher rate, he implies that there is a stimulus to which the response is made. The definition forces attention on the response itself rather than on the related stimulus. The major contribution of Premack's approach seems to rest on the objective criterion it provides for determining when a reinforcing stimulus will indeed be a reinforcing stimulus; namely, when the operant rate of response to the reinforcing stimulus is higher than the operant rate of the reinforced response.

The fact that far more space is allocated in this report to empirical findings than to theoretical issues is not intended to suggest that the theoretical issues are unimportant. The position taken here is that it is far more fruitful to look at the theories in terms of the data rather than to look at the data from the perspective of some particular theoretical orientation.

Chapter 2

Positive Reinforcing Stimuli: Consumables

Throughout this chapter we shall be looking at specific stimuli (or stimulus complexes) that have been demonstrated to result in a change in response frequency or strength. In addition, we shall give some consideration to the species used in the experiment, the measure of response change taken as a criterion for reinforcement, and the particular conditions under which the stimulus is indeed a "reinforcing stimulus."

The reinforcing effect of food and water for hungry and thirsty animals has been demonstrated innumerable times, being well enough established to form the backdrop of experimental psychology. But considerable attention still needs to be paid to this topic because it provides avenues of research which may well provide a key to the understanding of reinforcement phenomena. While the early studies of consumable reinforcements investigated the gross effects of a reinforcing situation on the frequency or strength of particular responses, the trend in recent years has been to differentiate aspects of the reinforcing stimuli as well as aspects of the response to these stimuli. Thus experiments have been conducted in which the food value of the reinforcer has been varied, as when the reinforcing value of saccharin solution which has zero food value is compared with sucrose solution. The concentration of solutions can also be varied. Recent studies have also varied the response aspects of the reinforcement process. Some attention has been given to the reinforcement which results from the absorbtion of food substances through the wall of the alimentary canal. The effect of the latter aspect of the reinforcing event has been compared with the reinforcing effect of the food finding, seizing, masticating, and swallowing aspects of the response. In addition, the effect of each of these aspects of the reinforcing event on a response is also dependent upon the condition of the organism at the time when the event occurs and also upon the species involved. The study of the separate factors which enter into the reinforcement event and the interactions among these factors is highly important for developing a theoretical formulation of the nature of reinforcement in the species studies. It does not follow, of course, that the theoretical formulations derived from studies of sub-human species can be applied, without modification, to the human species. But as the mechanisms of behavior are explicated in sub-human species, experience with other phenomena suggests that the same or similar mechanisms will be reflected in human behavior, the main differences resting on the relative importance of the particular mechanisms.

The term consumatory response occurs frequently in the literature on reinforcement, but it is one rather vague in meaning. In general, it refers to the last directly observable response in a chain of responses. Implicit in the idea of a consumatory response is that of a response which achieves an objective which satisfies a need. Thus the final response which results in the intake of food in the hungry animal is the consumatory response. Such object-need relationships are clear in the case of hunger, thirst, and sex, but much less clear in the case of curiosity, achievement need, and other psychogenic needs widely discussed in the literature.

When a rat runs down a straight alley maze to a goal box, the final response of eating and swallowing the food is referred to as a consumatory response. Just how far back the consumatory response extends in the chain of responses is rarely specified, but the custom is for earlier responses such as salivation and jaw movements which antedate the taking of food to be referred to as anticipatory responses. Just as there is no line drawn between anticipatory and consumatory responses, there is also an equal lack of definiteness in drawing a line between the consumatory response involved in, say, the taking of food, and the series of responses which follow and lead to the absorbtion of the products of digestion. Swallowing is generally included in the category of consumatory response while the contractions of the stomach are not. The consumatory response is, then, a part of a response sequence with very indefinite boundaries both at the beginning and at the end. Responses in this temporal region have been singled out for attention by psychologists because they have special significance in relation to reinforcement phenomena.

While the concept of a consumatory response has not been very adequately defined in food seeking and food ingesting behavior, it is an even more obscure concept in an area such as human problem solving. In the solution of an arithmetic problem, just what would one consider to be the consumatory response and what the anticipatory response? We ask this question because the concept of a consumatory response need not be limited to situations involving food, water, or sex.

Stimulus Properties of Consumables in Relation to Reinforcement

As early as the fourth century B. C., Aristippus had given formal expression to Hedonism as a psychological principle of behavior. Since that time, it has found many champions and is given current expression by several psychological theorists. Hedonism generally stresses the sensory qualities of the reinforcing stimulus, more particularly, those qualities that lead to pleasurable or painful responses. Although hedonic theories have been unable to explain behavior, they have provided principles that have emphasized the importance of the characteristics of the responding organism. In this sense, the interaction of the organism with its environment is emphasized. The importance of this emphasis is readily seen in the typical preference study.

Richter (1940) presented rats with two bottles fixed with drinking tubes. In a typical case, one bottle would contain tap water, and the other bottle would contain varying concentrations of some other solution. The measure of preference, then would be the relative amounts of the two solutions that were consumed by the rats. But the typical learning experiment is in many respects simply a special case of a preference study. Responding more frequently to the solution in one of two bottles is not fundamentally different from responding more frequently to one of two arms in a T maze. What is different, is that greater restriction is imposed upon the behavior of the animal in the learning experiment, both through motivational and physical restrictions, and the differences between the stimuli associated with the two arms of the maze are maximized, e. g. food as opposed to no food. In contrast to this, the preference study is normally conducted at a relatively low level of motivation; the substances are continually available,and are not highly dissimilar; and no direct control is exercised on the behavior of the rat. In both the preference study and the maze study, there is a change in the probability of response to either arm or bottle. This change is usually from a probability of .50 to something greater (or less, depending upon which is judged to be right). In this sense, the preference study is a more general case of a learning experiment, the preferred substance serving as the reinforcing stimulus.

Two important principles emerge from the preference studies. The first is that there is a definite relationship, if not an identity, between stimuli that hold some attraction for an animal and reinforcing stimuli. This principle if closely examined, would probably prove to be tautological,- it suffers from none of the same difficulties as the empirical law of effect, but nevertheless it seems worth stating.

Secondly, the reinforcing value of a stimulus depends upon the context of the stimulus. This is clearly demonstrated by data provided by Bare (1949). He found that rats, given continuous availability to solutions of saltwater in one bottle and tap water in the other showed no particular preference for one or the other with very weak solutions (less than .06 percent salt), but that with stronger saline solutions there was an increasing propensity to consume more of the saline solution up to concentrations of .9 percent, a concentration isotonic with the bodily fluids. At this concentration, the mean daily intake of saline solution constituted about 80 percent of the total fluid intake. At higher concentrations, the responsiveness to the saline solution diminished rapidly. At 1.5 percent there was an equal amount of saline solution and tap water ingested and at higher concentrations the amount of water ingested quickly exceeded the amount of salt water. Solutions of 2.5 percent resulted in nearly all of the water being taken from the container containing tap water. Again it might be emphasized that in order to respond to one bottle with greater frequency than the other, the animal must learn to discriminate the two. The solution to which the rats respond with greater frequency fulfills the role of reinforcing stimulus. Unfortunately, Bare recorded the amount ingested at 12-hour intervals, so it is not possible to show the development of the function. But this is similar to an accepted practice in learning experiments, where the analysis is performed on the first and last trials. It is the change in responsiveness that is important. It would also be interesting to see data on the number of approaches to the two bottles, but these, again, are not available. In spite of these deficiencies, it is clearly seen that the question of whether or not water is a reinforcing stimulus depends very much on the alternatives that are available to the creature. When there is a 0.9 percent solution of saline available, water fails to reinforce approach behavior. On the other hand, if the concentration of the saline solution is increased to 2.5 percent, water is very much a reinforcing stimulus - the behavior of the rats is shaped.

The inclusion of preference as a determinant of reinforcing efficacy has necessitated additional research on the correlates of preference. The question posed, generally, is this: Given a correspondence between preferred substances and reinforcing stimuli, what are the determinants of preference? Stated differently, why do the creatures approach some substances rather than others? Most investigations stimulated by these questions have explored the neurological and physiological correlates of the consumption of the substances.

It presently seems unlikely that variability of preference at different concentrations of substances will be accounted for in terms of the magnitude of the neural discharge that results from contact with the substance. Pfaffman (1960) plotted the magnitude of the chorda tympani nerve discharge in the rat at various concentrations of saline solution. He found this to be an increasing monotonic function throughout the range observed by Bare. While the data given by Bare (1949) showed that rats manifested a preference for saline solutions which increased up to the point at which they were isotonic with the body fluids and declined beyond that point, Pfaffman could relate preference to magnitude of nerve response only up to the point of maximum preference. Beyond that point preference decreased but neural discharge continued to increase. These data demonstrate that a preference for a particular solution is not simply a linear function of neural discharge, but must be attributed to other conditions. In addition, Pfaffman (1960) reported the behavioral and electrophysiological responses in the rat to sucrose, quinine, and acid (H Cl). Sucrose solutions show about the same relationship to neural discharge as sodium chloride, but at the optimal level (a molar concentration somewhat less than that for salt) they result in greater responsiveness to the solution than that observed for salt. Acid and quinine, above threshhold, are consumed in decreasing quantities until the intake at even fairly slight concentrations is negligable. In this respect, compared to tap water, they are probably negative reinforcing stimuli.

Pfaffman, attributing the reinforcing effect of consumables to sensory input, takes the position that sensory inputs may be either positive or negative reinforcers depending upon magnitude and the particular sensory nerve which has been activated and that they may also function to produce approach or withdrawal behavior. He also goes as far as to say that in the gustatory field there are stimuli which are reinforcers in their own right that are not dependent upon their relationship to drive reduction mechanisms for producing their effects upon behavior and learning. This is a position taken by an increasing number of experimental research workers and also one which has important implications for the interpretation of reinforcement phenomena.

Both consumatory and postingestive factors seem to account for the preference inversion reflected at concentrations of salt water which are isotonic with the bodily fluids of the rat. Stellar, Hyman, and Samet (1954) introduced strong saline solutions into the stomach of rats and found that this resulted in a decline in the preference for the more concentrated solutions, revealing the importance of the characteristics of the animal and postingestive factors. They also found that rats with an esophageal fistula showed the same preference inversion noted above, increasing responsiveness associated with increases in concentration of hypotonic solutions, but an aversion for hypertonic solutions of sodium cloride, after hypotonic solutions were introduced into the stomach through the fistula.

It would be a mistake to assume that the same mechanisms necessarily underlie both the reinforcing effect of consumables and the propensity of animals to approach them. It is quite possible, for instance, that post-ingestive factors exercise great control over the approach of consumatory behavior of the animal, while sensory factors in some manner are largely responsible for the reinforcing effect of the substance. It is not always possible to completely separate these two factors experimentally, but it is helpful to maintain the distinction conceptually since the relative importance of the two factors will vary considerably from one experimental situation to another.

Sheffield and Roby (1950) have emphasized the role of consumatory responses (or stimuli associated with consumption) in accounting for the effects of reinforcing stimuli. They ran rats in a T maze to a solution of saccharin and demonstrated an increasing number of correct responses, decreasing time to traverse the maze and an increasing rate of ingestion of the saccharin solution.

Since the post-ingestive factors associated with saccharin are considered to be inconsequential, especially in light of the very small quantities used, Sheffield and Roby attributed the reinforcing effect of saccharin to the sensory qualities associated with it. Since there is a strong tendency for Hullian Drive theorists to interpret an effect of this sort in terms of the secondary reward value (whereby a stimulus has reward value because some characteristic of the stimulus has been previously associated with primary drive reduction), Sheffield and Roby gave specific attention to this possibility. They observed that this alternative interpretation seemed implausible for two reasons. In the first place, the reinforcing value of the saccharin did not extinguish over repeated trials as one would expect of a secondary reward in the absence of primary drive reduction. Secondly, they noted that saccharin is probably much sweeter than food previously experienced and associated with drive reduction in the conversion of dietary starches into sugar. If this is so, then saccharin would be in a very unfavorable position on a gradient of generalization and the reinforcing effect would not be expected to be as great as it was demonstrated to be.

Hutt (1954) considered the same problem and offered supportive data. He trained rats to respond in a Skinner box. After training, he randomly assigned the rats to nine different conditions, three dipper sizes for the basic food (a mixture of milk, flour, and water) distributed across basic food, basic food with saccharin, and basic food with citric acid. On a 23 1/2 hour food-deprivation schedule, the rats were placed on a one-minute fixed interval reinforcement schedule and the cumulative bar-pressing was recorded. Both taste and amount were significant ($p<.005$) in asymptotic rate of bar pressing, the sweet (saccharin added) food in large amounts resulting in the highest rate. The small amount with citric acid yielded the lowest rate of responding. As additional evidence of the reinforcing effect, he reported a correlation of .90 between rate of responding during the reinforced trials and the number of responses during extinction.

In an additional study (Guttman, 1954) of the sensory qualities associated with reinforcement, the reinforcing effects of sucrose and glucose were compared. The importance of such a comparison rests on the fact that while the nutritive characteristics of the two are essentially the same, sucrose, at least for human judges, is sweeter than glucose. If the nutritive or drive reducing qualities of the substances are the critical factor in reinforcement, then the rates of response to the two solutions would be the same at any given concentration. On the other hand, if the sensory qualities are more important, then equal rates of response would be expected only for concentrations of the two solutions that were matched on an equal sweetness basis. In other words, it would take a greater concentration of glucose to produce the same rate of response as is obtained with a given concentration of sucrose, since the latter is presumed to be sweeter for rats as it is for men. This is essentially what Guttman found. He used a periodic (one-minute mean interreinforcement) schedule for concentrations of sucrose and glucose ranging from two percent to 32 percent solutions, and found that the rate of response to the sucrose was consistently higher than the rate of response to glucose. Further, he found that the differences in rate of response to the two solutions did not depart appreciably from what would be expected on the basis of an isosweetness criterion for concentrations up to about 15 percent solutions. Above this point there is more disparity between his data and the data accumulated on isosweetness. The significance of the finding gains by virtue of the fact that all rats used in this experiment had had extensive previous experience with both sucrose and glucose during periods of food deprivation, so there had been ample opportunity for the rats to associate the experimental solutions with their drive reducing consequences. Especially in light of this fact, Guttman gives considerable support to the position that the reinforcing effect of a reinforcing stimulus is closely associated with the effects of the stimulus upon the receptor mechanisms of the animal.

Sheffield, Roby and Campbell (1954) have also investigated this problem. They separated the gustatory and postingestive factors by using one stimulus, saccharin, that is very sweet but not particularly noted for its postingestive effects, and another, anhydrous dextrose, that is very nourishing but is not very sweet. In addition to these two treatments, they used water and a mixture of anhydrous dextrose and saccharin to serve for control. For these four reinforcing conditions, they placed four groups of six rats on a hunger regime of eight grams per day for six days and then ran the rats in a runway, recording both speed and the amount of the solution ingested while the rat was in the goal box. The sweetest solution, the combination of dextrose and saccharin, resulted in the fastest running speed and also in the greatest amount of consumatory behavior. This was followed fairly closely by the effects of saccharin, while the reinforcing effect of anhydrous dextrose was not much better than that of water. In a second experiment, the procedure was not modified, but the animals were given pretraining in running either to dextrose or to water. This was introduced to afford the rats an opportunity to associate the stimulus characteristics of the dextrose with its drive-reducing properties. While this procedure resulted in an initial advantage in running speed for the animals that had had the pre-experience with dextrose, it also resulted in a greater amount being consumed in the goal box. The rats receiving water during the pretraining trials overcame the advantage and equalled their asymptotic running speed during the experiment proper. Further, the experimenters found running speed to be nearly a linear function of the amount of reinforcing solution consumed. Thus they concluded that consummatory behavior is of far greater significance in reinforcement than is drive reduction.

In addition to the strategy of varying sweetness while holding drive reduction constant, another technique for separating the gustatory and drive reduction aspects of reinforcement has been to provide taste stimulation with quantities so small that little drive reduction could be expected to occur. This is the technique employed by Smith and Kinney (1956) and Smith and Duffy (1957). In the first experiment, sixteen drops of either a twenty percent sucrose solution or water were given to hungry and sated rats as reinforcement for a lever response during a 46 minute test period. The total number of responses during the test period was the dependent variable. In a subsequent analysis of variance, it was shown that a drop of sucrose had a significant reinforcing effect (P=.01), but that there was no significant difference between hungry and nonhungry groups. The lack of difference between hunger groups may be in part due to the fact there was a significant interaction between the condition of hungry or nonhungry and the nature of the reinforcer; this was mainly a result of the fact that hungry rats made fewer responses for water reinforcement than did the sated rats.

In the second experiment in the series (Smith and Duffy, 1957) the same reinforcing solutions were used, but the animals were run in a T maze. In the first part of the experiment, one arm contained one drop of a 20 percent sucrose solution, while the other arm, for the purpose of direct comparison, contained one drop of water. In order to provide an opportunity for drive reduction to occur, a second group of rats were offered four cubic centimeters of the reinforcing solutions in the maze. A drive reduction effect, then, would be revealed in a greater difference between sugar and water in the second part of the experiment; in addition to the difference in taste, a difference in drive reduction would be effective. They also used both food deprived and sated rats in both parts of the experiment as a further control on the role of drive reduction, for the effect would presumably be more marked in the food-deprived animals.

All of the rats used in the experiment received two trials each day, one free and the other forced to the side neglected on the free trial. The data were collected on the free trials and were analyzed in terms of both response latencies and the number of correct trials. In terms of both measures, the hungry rats were superior to the satiated rats when they were offered four cubic centimeters of sugar solution. But the hungry animals made only slightly more correct responses than satiated animals when reinforced with only one drop, and they also had longer latencies in response. For the purpose of avoiding ambiguity of results, Smith and Duffy ran a check experiment on this phase and demonstrated that hungry and satiated rats did not differ on either measure when reinforced with one drop of a twenty percent sugar solution, but both showed improvement in terms of an increase in the number of correct responses and a decrease in response latency. They offered these results as evidence of dual reinforcing effect for sugar. Drive reduction was presumably operative in the food deprived rats which were run to four cubic centimenters, thus accounting for their superiority. But with smaller amounts, e. g., one drop, only the stimuli associated with consumption were available for reinforcing the behavior.

These results strongly suggest that a sweet substance may have innate reinforcing qualities; the reinforcing effect is somewhat independent of previous experience in which the substance has been associated with primary drive reduction, although this latter type of experience may contribute to the reinforcing effect.

There are several observations to be made on the studies that have been considered up to this point. One of the more important is that the reinforcing efficacy of a substance is highly dependent upon the stimulus context. Whether or not a particular substance will reinforce a response depends highly upon what other substances are available in the environment. This is clearly demonstrated in the experiments conducted by Richter (1940) and Bare (1949)and gathers considerable support from the data provided by Smith and Duffy (1957).

Further, it has been possible to evaluate the hedonic aspect of reinforcement, at least in limited cases. The evidence of this issue is accumulated from a great number of sources. Guttman (1954) demonstrated that equal reinforcing value of sucrose and glucose is obtained at concentrations, generally, that are of equal sweetness rather than of equal nutritive value. Sheffield, Roby, and Campbell (1954) did extensive work using saccharin, a non-nutritive sweet, as a reinforcer and found that it had a greater reinforcing effect than less sweet nutritive substances. A third major technique for the investigation of this problem has involved the use of quantities that are so small as to be of negligable drive-reducing value (Smith and Kinney, 1956; Smith and Duffy, 1957). These studies also support the position that factors alone, the qualitative aspects of the stimuli, have the capability of determining whether a consumatory response will have positive or negative reinforcing value.

Just as Hull (1943) based his argument for a drive-reduction mechanism of reinforcement on the survival value of such a mechanism, Woodworth (1958), among others, has suggested that a hedonic mechanism would also contribute to the survival of the species and thus would have a selective advantage in evolutionary development. This argument is based mainly on the fact that sweet vegetation tends generally to be edible (and nourishing), while bitter tasting plants are likely to be poisonous.

Postingestive properties of consumables in relationship to reinforcement

While it seems fairly well demonstrated that the stimuli associated with consumatory responses have reinforcing properties that are independent of the post-ingestive effects of the substance, it still remains a real possibility that the converse is true, that post-ingestive effects (e. g.

drive-reduction) can have some control over the reinforcing efficacy of a substance that is independent of the hedonic qualities.

The data provided by Smith and Duffy (1957) that was reviewed in the last section gives some support to the position that there is a dual reinforcing mechanism, that drive-reduction may contribute to the reinforcing value of a substance, and further support comes from other quarters.

The major techniques for this research have involved the experimental separation of the consumatory and postingestive factors, either by innervating the sensory mechanisms or by directly introducing the reinforcing substances into the internal environment. This latter technique is exemplified by the work of Miller and Kessen (1952). They prepared experimental rats with fistulas that permitted the introduction of milk directly into the stomachs of the animals. Then using a standard T maze, they reinforced a correct response with 14 cc. of enriched milk and, as control, injected 14 cc. of saline solution when the rat made a wrong response. Under these conditions, the rats reached a high level of performance as determined by the number of correct responses. Since the consumatory act had been bypassed, this source of stimulation can be discarded as a possible interpretation of the results. The difference in performance can be attributed to the difference between saline and enriched milk. This lends some support to the drive-reduction hypothesis since one of the conspicuous differences between the two is the difference in nutritional value, but as will be seen later, there are also other differences that might account for the results.

Another experiment that was conducted as a direct test of the drive reduction hypothesis was reported by Thomson and Porter (1953). Instead of bypassing the sensory mechanisms, they severed sensory nerves to make, the rats both ageusic (lacking in taste sensation) and anosmic (lacking in smell sensations), hence eliminating these sensory qualities as potential sources of reinforcement. They then induced a condition of sodium deficiency by feeding the rats on a special diet and used dilute (sub-threshold) solutions of saline solution as a reinforcing stimulus. They experienced considerable difficulty in gaining evidence of learning. It was necessary to provide extensive opportunity for the rats to associate the saline with drive-reduction by forcing trials to the saline, but, with a modified experimental procedure, there was some evidence that drive-reduction had functioned as a reinforcing mechanism. Unfortunately, post-mortem examination showed some evidence of regeneration of the sensory nerves. Since they had not conducted preference studies to determine whether or not the solutions used to reinforce the response were still sub-threshold at the conclusion of the experiment, the results are suggestive but inconclusive.

Coppock and Chambers (1954) devised an apparatus such that the rat confined therein could control the intravenous injection of either glucose or saline by the location of its head. A pretest determined the non-preferred head position and thereafter glucose was automatically injected while the head was in that position. If the head moved to some other position, the apparatus automatically switched to the saline solution for injection. The glucose was effective in increasing the relative amount of time spent with the head in the reinforced position.

Chambers (1956a) devised a test apparatus that would also automatically control injections. He used an iron cylinder 23 in. wide and 40 in. high with a floor consisting of seven plastic covered plates of equal size. The plates were pressure sensitive and were connected to pen recorders and the injection apparatus. A pretest period was used in this experiment also to determine the non-preferred position in the cylinder for rabbits having undergone six, four, three or zero days of food deprivation. The apparatus was set so that the rabbits would receive an intravenous injection of a solution of glucose if the animal was on the least preferred plate, an injection of a solution of xylose if on the preferred plate, and normal saline on the remaining five plates. The results showed the rabbits spending significantly more time on the previously non-preferred plate while there was no effect attributable to the intravenous injection of xylose. The effect of the injection is complicated by the fact that the increase in time spent on the reinforced plate did not vary directly with the amount of food deprivation. The rabbits starved four days showed the greatest change in performance, excelling those starved either six or three days and those not subjected to food deprivation.

Direct injection of food substances into the blood stream has not always produced positive reinforcement. Chambers (1956a) used dogs that had been deprived of food for one day or not at all in the same sort of experimental arrangement and failed to find an appreciable difference between the experimental and control group in the distribution of the time spent on the glucose-producing plate. He reproduced his previous experiment in most essentials, but this time used dogs that had been starved for three days and again failed to find evidence of learning, although one beagle pup did show proportionate increase in the time spent on the plate that resulted in glucose being injected intravenously.

While it has been adequately demonstrated that intravenous injections result in a decrement of drive as measured by consumatory and instrumental responses, the learning in the previously cited studies are not necessarily attributable to drive reduction, for injections have other demonstrable effects. Chambers (1956b) investigated these other physiological effects of intravenous injections into rabbits. He randomly assigned 12 rabbits to treatments and starved them for four days. Following starvation, the rabbits were given 80 minutes for adaptation to the confinement apparatus and then were given an intravenous injection of glucose, xylose, or saline. He recorded the temperature of the rabbits at the rectum and on the back, ear and leg, both pre-and post-injection. The results showed generally a greater temperature rise for glucose than for injections of xylose or saline, and the differences were significant at the .01 level of confidence for a temperature recordings taken on the back and the leg. The main effect occurred at five minutes (the first reported measure) past the time of injection. All injections were of a standard quality based upon 1.5 cc/kg. body weight. Temperature change may be one factor associated with the reinforcing properties of intravenous injection.

Experience and the Development of Secondary Reinforcing Effects

The effect of the consummatory response as a reinforcer has been ascribed by drive reduction theorists to the secondary reinforcing properties acquired by this response through its temporal association with drive reduction. Sheffield and Roby (1950) discarded the drive reduction position in their original study with saccharine as a reinforcer on the grounds that the reinforcing properties of saccharine did not appear to extinguish as they would if they were secondary rather than primary. An experimental approach to this problem is to study the reinforcing property of a sugar solution in rats that have had experience with sugars and rats that had not. The ansumption in such studies is that a rat that has had experience in consuming sugar has learned to associate the consumatory responses with the consequent drive reduction. For rats that have had such experience, the reinforcing properties of sugar should be stronger than those that have not.

In a study by Sheffield, Roby and Campbell (1954) four groups of six rats each were placed on a regular starvation regime of 22 hours of fasting followed by the consumption of 8 grains of purina mash. Twelve rats then had experience with runway training with dextrose as a reinforcer, while the other twelve rats did not. Finally, all rats were trained in a straight alley maze with dextrose provided in the goal box. The outcome of the experiment was that no marked differences were found between the two groups of 12 rats though some initial advantage was observed in the rats that had had previous training with dextrose.

A more critical test of the effect of experience was conducted by Wetzel (1959). Rather than pretraining trials, he made a solution of 20 percent sugar available in Richter tubes in the home cage for fifteen minutes a day for 11 days. During this experience, the animals were about 22 hours food deprived. Control rats were placed on the same schedule, but received water rather than sugar. Following this, the rats were divided into hungry and satiated groups and received training trials in a T maze which had 1 drop of sugar solution on one side. The hungry group which had had previous experience with the solution excelled the performance of the other three groups. There were no significant differences between the other three groups, although all three showed evidence of having learned the correct response. These results demonstrate that hunger facilitated the effect of experience with a sugar solution as well as demonstrating the effect of experience on certain classes of reinforcers.

Overview of Research on Consumables as Reinforcers

The study of consumables as reinforcers represents the area of reinforcement learning in which the most systematic and concentrated effort has been made to determine what such learning involves. Much of this research finds its origin in a drive reduction theory of learning and represents attempts to test hypotheses related to this theory. That the phenomena involved are, beyond question, extremely complex is well attested by the research and this complexity justifies fully approaching the study of reinforcement through the study of the consumption of edible and potable substances where some of the variables involved can be easily measured. Such substances permit not only a high degree of control over the stimuli involved, but they also permit the isolation and separate study of the anticipatory phases, the consumatory phases and the post consumatory phases of the reinforcement process.

A substantial amount of the research supports a drive-reduction theory of reinforcement. Under some conditions, gustatory stimulation may either acquire its power to reinforce or may have its power of reinforcement enhanced through its relation to the subsequent ingestion of food. This aspect of the evidence fits the drive reduction paradigm and the theory that taste stimulation, if it functions as a reinforcer, is a secondary reinforcer which has acquired its properties through pairing with subsequent drive reduction events. The parsimonious nature of such a theory is tantalizing, but not all of the evidence fits. Particularly damaging are the findings with physiologically inert substances such as saccharin and the comparison of the reinforcing properties of those substances which, for example, have equal drive reducing properties such as sucrose and glucose, but which are not equally sweet in terms of human judgments.

The drive reduction theorist has a comeback to the criticism that saccharin, having no drive reduction properties, cannot function as a reinforcer except through its gustatory properties. He points out that saccharin may well owe its properties to its taste similarity to sugar which has drive reduction properties. However, if this were so, one would expect the effects of saccharin as a reinforcer would be rapidly extinguished, but such is not the case. Saccharin can be used almost indefinitely as a reinforcer. There is, of course, the possibility, as Mowrer (1960) has pointed out that under some conditions secondary reinforcers may represent highly stable characteristics which are resistant to extinction.

While the theory that taste stimuli may function only as secondary reinforcers still has considerable support, there is a growing tendency among research workers to take the position that this is only a part of the story. Research workers are showing, in the discussions of their data, an increasing tendency to flirt with the idea that taste stimulation may represent a primary reinforcer which does not function through a drive reduction mechanism. The sentiment commonly expressed is that the drive reduction theory of reinforcement has to be stretched more and more to be compatible with research results, and that attempts to account for results in terms of such a theory go far beyond the limits of plausibility. While nobody can deny that the drive reduction representation of reinforcement fits much of the research data, there are strong possibilities that other mechanisms may also be involved. Some writers freely discuss what they refer to as an hedonistic mechanism operating in the case of some reinforcements. Such a conception with its phenomenological implications does not represent an approach too acceptable to experimentalists, but there are alternative conceptions which may well be acceptable. One of these is the conception developed by Woodworth of the primary of behavior and the need for the organism to act on the environment with resulting feedback of stimulation. If such a need is inborn, then any source of stimulation may have reinforcing properties in that it directly satisfies a basic need. Such need satisfaction may have, in itself, hedonic qualities. To a great extent, the research to be discussed in subsequent sections, which explores the reinforcing properties of a wide range of stimuli, is based on these emerging ideas and represents a departure from the drive reduction approach involving the traditional list of primary drives.

Chapter 3

Positive Reinforcing Stimuli: Exteroceptive Stimuli

Laboratory workers who conduct research with animals are all familiar with what has been called spontaneous behavior, that is to say activity that is not obviously related to any particular condition of deprivation nor to any especially discernible aspects of the environment. Animals that are well fed and watered do not typically curl up and go to sleep; rather, a high level of activity is the rule: they explore their environment, investigate new stimuli, engage in play, or simply dissipate energy on an activity wheel if one happens to be available. In addition, it is easily noticed, if one observes a creature for any length of time, that there are sequences of behavior that tend to be repeated; activities are performed time and time again that have no obvious source of reinforcement, at least not in terms of Hullian theory (Hull, 1943). Finally, a careful observer will be impressed with the variability of behavior. A hamster can be seen to run in an activity wheel, climb the side of the cage, preen, run around the perimeter of the cage and return to the activity wheel again in the space of a few short seconds. It will not typically persist in any one activity for a very long time.

The term spontaneity is not intended to denote behavior that is in any sense random (Meyer (1924) admonished us long ago on the use of this term), but is used instead to designate behavior that is relatively unexpected or unpredictable. The determinants of such behavior, whatever they are, are unknown. It is true, of course, that the creatures fail to conform to the popular notions that activity is the result of drive stimuli and that behavior is shaped by the action of reinforcement, but this seems more an indictment against the notions than an indictment against the rat. It appears, as a matter of fact, as though the animals are in some manner attracted from without as well as driven from within. In addition, the interaction of the animal with its environment seems itself to be a determinant of subsequent behavior, a factor that is important in determining the variability of behavior.

The interactions of the organism with its environment which are considered in this chapter are important to the understanding of reinforcement phenomena in two ways; first, so-called spontaneous behavior which appears to fit the reinforcement paradigm and, second, the behavior itself or the stimuli associated with it may sometimes provide reinforcing conditions. While all of the investigators have in one way or another been concerned with the variability of behavior, there has been a tendency to at least conceptually organize the field under four separate headings; spontaneous alternation, exploratory tendency, activity drive and curiosity drive. As the chapter progresses, it will become apparent that it is far easier to separate these areas conceptually than it is to separate them experimentally, but then part of the concern is to show the relationships among them.

Spontaneous Alternation

Spontaneous alternation, as has been suggested above, is a form of variability in behavior that has in no way been solicited. It is perhaps a tribute to psychological methods that a form of behavior that, by definition, leads to a lack of uniformity in behavior should lead to uniformity in results, for this is exactly what has happened. Tolman (1925) was the first to report this. He observed rather incidentally that his rats tended to alternate their responses in a T maze. If they went to the right side of the maze on the first trial, then they would go to the left on the second trial. Conversely, a trip to the left was usually followed by a trip to the right. Tolman, himself, was not particularly concerned by this. He predicated his position upon an exploratory tendency and simply regarded this as exploratory behavior in a two-choice situation. As a matter of fact he did not bother to publish the finding separately, but only casually mentioned the fact in a theoretical paper. For this reason, his particular finding was lost for 14 years.

A variant form of alternation was recorded by Thorndike (1927) at about the same time. Thorndike, as in many of his experiments, read a list of words to human subjects who responded to each word by saying a number from one to nine. The subjects on the first round could only guess at which number was right. Thorndike observed that subjects very rarely ever gave the same number twice, and indeed, they gave the same number consecutively less frequently than would be expected by chance. Thorndike likened the phenomenon to the refractory phase which follows the transmittal of a nerve impulse and which prevents the immediate passage of another impulse along the same nerve fiber. The study by Thorndike was repeated by Telford (1931) and Luh (1931) with some attempt to control the interval between successive stimuli. Both of the latter research workers found that as the interval between successive stimuli was decreased, there was an increasing tendency to avoid repeating the same response twice in succession. However, with longer intervals of time the responses demonstrated a tendency to show some ordering with subjects tending to give numbers in sequence. Luh reported that subjects indicated a conscious bias not to produce the same numbers on successive occasions.

The similarity of the phenomenon to the psychological concept of a refractory phase made the area an attractive one for research during the thirties. Irwin and Preston (1937) made the interesting observation that subjects who judged the second of two sounds to be the louder tended to avoid judging the second of two weights to be the heavier if these two tasks followed one another. The fact that this phenomenon occurred across sense modalities was of considerable interest and suggested that a high degree of generalization of the phenomenon was possible. In another study by Telford and Thompson (1933), data supported the hypothesis that if the same reflex were to be initiated with short intervals between successive stimuli that the amplitude of response showed a decline. While Telford and Thompson considered this to be related to the avoidance phenomenon demonstrated by Thorndike, it is doubtful whether there is any direct relationship.

While there are obvious similarities between the studies reported by Tolman and Thorndike, there are equally obvious differences. Tolman, of course, had used rats while the remaining experiments had used human subjects. Further, the stimulus and the response were correlated in the Tolman study. As the rat ran down the stem of the T and came to the choice point, a right turn was associated with the right arm, while a left turn was paired with the left arm. A change of response, then, could be interpreted as a case of stimulus change, or, in Tolman's terms, stimulus avoidance. This interpretation is not possible in the study reported by Thorndike, for the successive responses were made to different stimuli. As Luh reported, the variability on the part of the human subjects seemed to be a function of a conscious bias against making the same response of successive occasions.

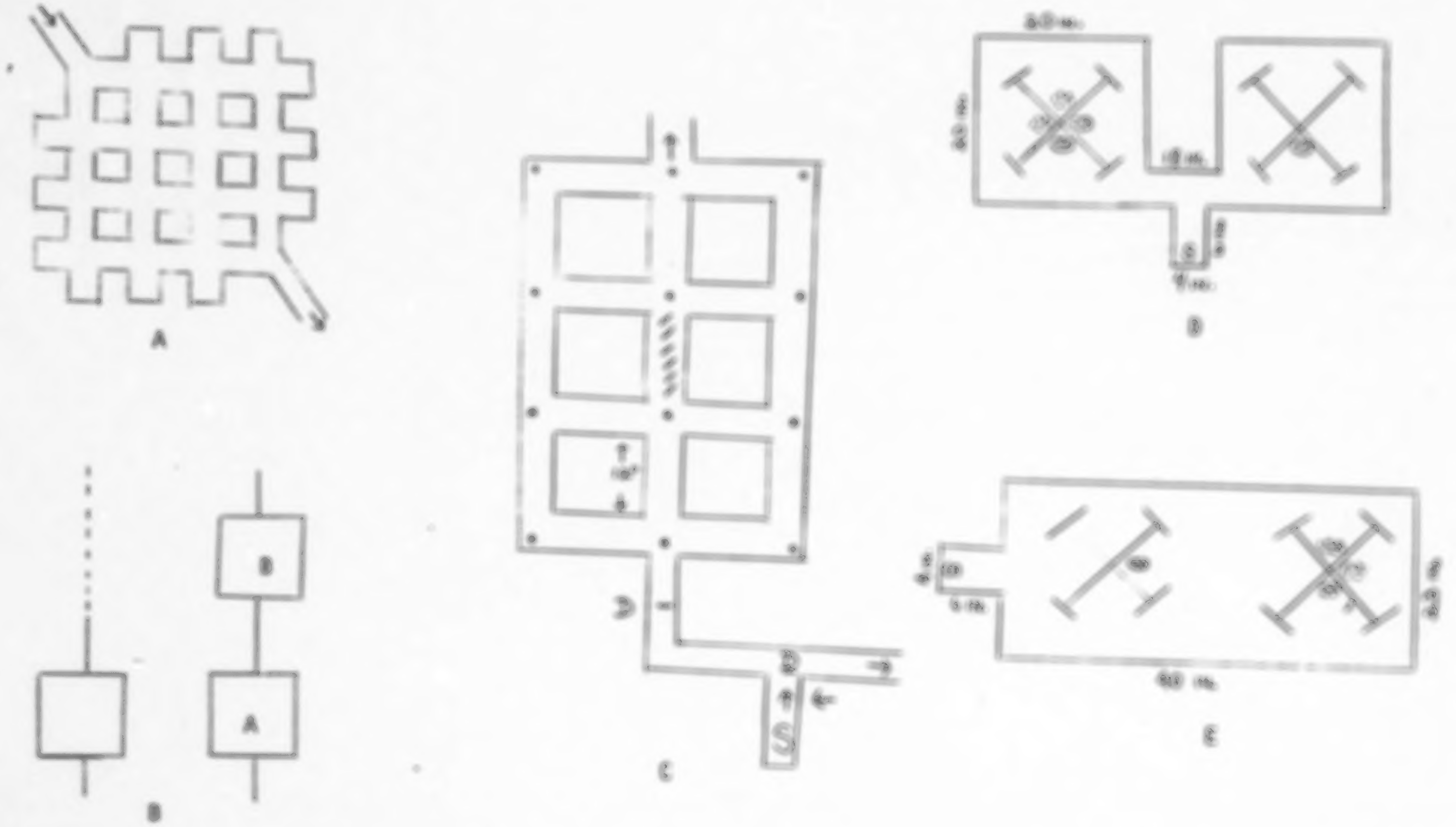

Fig. 1.1 A. Dashiell Maze (from Dashiell, 1925) B. One unit and two unit elevated mazes; path indicated by lines (from Dennis, 1930) C. Enclosed maze affording variable paths by appropriate blocking (from Krechevsky, 1937b) D. & E. Problem boxes; rats start from stem (from Havelka, 1956).

Additional data on this point is reported in a later section.

Dashiell (1930) provided the stimulus for further work on the variability of behavior. In a celebrated study on direction orientation in the rat, he had used a maze (which has subsequently taken his name in recognition of his pioneering efforts) that afforded the rat alternative routes to the goal box (Fig. 3.1a). His major finding was that the rats repeatedly made errorless runs to the goal box, but used different paths on successive trials. In short, the rats did not stabilize on a fixed route to the goal. This study was repeated by Peters and McLean (1935) who used college students for subjects and a finger-maze replica of the Dashiell maze. It is not particularly surprising that they failed to obtain the same results as Dashiell, but it is interesting to note that response sequencing did occur in a situation where the subject has been simply instructed to get to the end.

Krechevsky (1937a, 1937b, 1937c) conducted a series of investigations on the relationship of brain mechanisms to variability in behavior. In the first study, he used a Dashiell maze with normal and operated rats, the operated rats suffering brain lesions of varying size and location. He observed that the normal rats made far fewer errors than did the operated rats when running through the maze to food, and also that they made far more shifts in the path used in traversing the maze. All 20 possible paths through the maze were used by the normal rats, but he noted that the normal rats, contrary to the results obtained by Dashiell (1930), eventually settled down to an alternation route (in the case of the Dashiell maze this is the most direct path from the start box to the goal) through the center of the maze. This does no violence to an interpretation based on an exploratory tendency, for with both a hunger drive and exploratory tendencies operating in the initial trials, both could be satisfied by making variable, but errorless runs during the early trials of the series. As the maze was more fully explored, there would be a decrement in the exploratory tendency and the hunger drive would predominate. The task then changes to one of getting through the maze as efficiently as possible. Assuming this interpretation, the conclusion would be that exploratory behavior decreases as a function of familiarity with the maze.

The second study conducted by Krechevsky (1937b) started the rats in a T, but had two different units attached to the arms of the T. One afforded a fixed, unvarying path to a goal box, while the other (see Fig. 31b) provided an arrangement such that, by inserting appropriate gates, the path was varied from trial to trial. The normal rats elected to use the variable path on 57 percent of the trials even though that path was 20 percent longer than the fixed path. This compared with 34.3 percent for the operated rats. When Krechevsky (1937a) increased the length of the fixed route to the point that it was longer than the variable path by a ratio of 1.54 to one, he found no significant difference in the behavior of the normal rats, but found that the operated rats then switched their preference for the shorter route to the point that they selected it 51.3 percent of the time.

Shortly after the appearance of Dashiell's monograph, there were several studies on the importance of a directional orientation in the behavior of the rat. Dennis (1931) and Dennis and Henneman (1932) had demonstrated that the initial maze behavior of the rat was not simply random, for the rats consistently made fewer errors than would be expected by chance. Since there is no opportunity for proprioceptive factors to lead to partial success on the first trial, Dennis initiated a series of studies on directional orientation. This resulted in the studies reported by Dennis, Henneman and Varner (1932) and Wingfield and Dennis (1934). In the second of these studies, two paths on an elevated maze led to food. One path started toward the goal and the other path initially led away from the goal, although the distance by the two routes was equal. With one trial daily, the investigators found a clear and persistent preference for the path that led toward the goal, but when they used two trials daily (to see what would happen), they learned, much to their surprise, that the rats alternated paths on the two trials. In an experiment to test these results on alternation, they gave 13 rats two trials daily for 14 days. Of the 182 pairs of trials, only 13 were run on the same path. Over 90 percent of trials demonstrated alternation behavior. Following these results, Dennis conducted further studies on this particular phenomenon (Dennis and Sollenberger, 1934, and Dennis, 1935). In these studies, he found that spontaneous alternation persisted in other types of mazes, and found also that exploration of a maze diminished the tendency toward further exploration. He suggested that the importance of reward in maze learning was not that of directly reinforcing the correct response, but was simply a matter of off-setting the effect of negative adaptation. Without reward, the rat would eventually fail to run in the maze at all.

Dennis (1939) finally used an elevated maze (Fig. 3.1c) that consisted of an elevated square with opposing arms attached. He again found that alternation occurred with a one unit maze. When he used two units (also shown in Fig. 3.1c), he found that the response at the second unit was independent of the response made to the first unit, but was a function of the response made at the second unit on the first trial. Similarly, the responses to the first unit were alternated, but were independent of the responses made to the second unit. This, incidentally, is the first evidence cited in the literature that suggests the 'place' rather than response is alternated, contrary to the Hullian principle of reactive inhibition (Hull, 1943). On the basis of reactive inhibition, the response to the second unit would be expected to be a function of the response made to the first unit. When Dennis added a third unit to the arrangement mentioned above, the amount of alternation decreased appreciably. With ten units, the amount of alternation decreased to a chance level.

These studies adequately demonstrate that stimulus conditions have a persistent effect (e.g. during the period separating the two trials), that the major effect is to result in a decreased probability of response to the stimuli, and that reactive inhibition does not adequately account for the results. Since time between stimuli varied with the number of units in the last study, this may also be important. Wingfield (1943) attempted to extend the generality of the results to children, but failed, in this instance, to find the effect. He laid out two separate paths to a goal chair (on which he placed a piece of candy) with 1" X 12" boards. The child, then, was placed on a starting chair that was contiguous with both paths and permitted to run the maze to the goal chair. Following the first trial, the child was picked up (fortunately, the children were small) and placed back on the start chair for the second trial. In discussing the results of the experiment, Wingfield suggested, as was mentioned above, that the children were highly motivated to return to the outdoors with their playmates and that this motivation masked an intrinsic motivating properties the maze may have had for the children. In support of this interpretation, he mentioned that in an earlier unpublished study, students had tended to alternate cards when asked to pick up one of two cards that were presented in pairs. They would pick-up the left card on one trial and the right card on the next trial. But when he asked them to do it as quickly as possible, there was no longer any alternation. The response (picking up the left card or right card) became fixed. In addition, it might be suggested that the stimuli associated with the boards provide relatively less of the effective stimulation for the child. The maze may constitute a relatively large part of the rat's environment.

Heathers (1940) investigated the importance of time between trials and found that this was an important variable. He found that alternation on an elevated T maze occurred as high as 83.6 percent of the time with a 15 second inter-trial interval, but only 65.6 percent when the interval was increased to 120 seconds. Montgomery (1951b) conducted an additional study on the time between trials. Also, as a more direct test of Hull's ninth postulate, he installed levers at the end of the T maze to which varying weights could be attached. He then varied the inter-trial interval and bar weight independently and measured the amount of spontaneous alternation as a function of these two variables. He found that the number of trials and the inter-trial interval affected the amount of alternation (alternation decreased as a function of increase in both), but that alternation behavior was independent of the bar weight (or effort required). These results are in accord with the results reported by Dennis (cited above) and are surely in accord with results that will be reported in connection with exploratory behavior.

It was mentioned at the beginning of this section that although the areas of spontaneous alternation and exploratory behavior are easily separated conceptually, it is more difficult to impose the distinction on the experimental results. There are many cases where one concept fits as well as the other, and probably more where spontaneous alternation seems to be, as Tolman treated it, a case of exploratory behavior in a two-choice situation. Since there are many experimental results to be reported that have bearing on one as well as the other, no effort will be made from here on to maintain the distinction. Up to this point, it has been shown that many authors, especially Dennis, have discussed spontaneous alternation in terms of exploratory tendencies; Montgomery became the major proponent of the related concept of an exploratory drive.

Exploratory Behavior

The study of exploratory behavior, like the study of spontaneous alternation, has early origins. Many of the naturalistic observations that characterized the nineteenth century have reports of exploratory behavior, but during the first quarter of the present century, as naturalistic observation gave way, at least among psychologists, to controlled experiments, there was both decreasing opportunity for it to occur (except as it showed itself in alternation) and decreasing interest in that particular type of behavior. Drive theory was becoming formalized in psychological laboratories; complex forms of behavior were being reduced (hopefully) to their simpler components.

Moss (1942) had succeeded in measuring drive by measuring the amount of resistance (in terms of electrical current) that an animal would overcome in order to achieve an appropriate goal object. Animals were, it would seem, goaded into activity by drive and directed by the law of effect.

It was, as a matter of fact, during a demonstration of animal drive that Dashiell (1925) secondarily demonstrated the persistence of exploratory tendencies. Theory required that hungry animals be activated. In order to quantify the amount of activity that would result from this deprivation, he developed his famous maze (Fig. 3.1a). His measure was the number of squares an animal would traverse in a specified period of time. Since he was well aware of the tendency for rats to explore a new maze rather extensively, he attempted to minimize this effect by exposing the animals to the maze each day for a period of two weeks preceding the experiment. In this manner, he could maximize the difference in exploratory tendency between hungry and satiated rats. While he succeeded in demonstrating greater activity on the part of the hungry rats (this result has been subsequently disputed), he also found that the well-fed animals also engaged in extensive exploratory behavior.

The next study of exploratory behavior was one of several studies of drive directed by Warden at Columbia. The basic technique used in the series of experiments was the Columbia Obstruction Method, the use of an electrified grid, again, in order to measure the strength of a drive. In the experiment of immediate interest, Nissen (1930) found that rats would cross an electrified grid in order to gain access to a Dashiell maze. He had found in the past that wood shavings, blocks of wood, corks etc. held some attraction for rats, so he added these in order to further increase the interest value of the maze. A further control which will be of more concern in a later section was promoted by his conviction that there was an independent activity drive. Since he wanted the rats to be drawn by the maze rather than shoved by their drive, he provided large home cages that afforded ample opportunity for activity. With these provisions, he found a large number of crossings of the electrified grid. This suggests, of course, that events underlying exploratory behavior have drive properties, a matter that will be discussed in greater length at a later time. It is important, however, to note, if for no other reason than to account for the general lack of experimentation with exploratory behavior during the period to 1950, that Nissen's was a dissenting voice. The relationship and relative importance of intra-organic and environmental sources of stimuli had been frequently discussed in relationship to drive, and while it was generally acknowledged that an animal did not respond in a vacuum, the importance of environmental stimuli was generally minimized. Even as early as 1930, Hullian principles were directing the mainstream of psychological research. For reasons of parsimony, mechanisms were being held to a minimum.

For the reasons cited above, the large bulk of systematic investigations of exploratory behavior (and related phenomena) have occurred during the last decade. Most of these studies have been directed toward the elaboration of the determinants of this type of behavior. To phrase the matter differently, it has long been realized that rats are curious (see, for example, Small (1900)) to the extent that they will behave in such a manner as to bring themselves into contact with new stimuli introduced into their environment, but surprisingly little has been known about the dynamics of this sort of behavior. Is the behavior native or learned? Will any new stimulus have this effect? Are there important species differences? How important are responses to novelty in the general control of an animal's behavior? Answers to these and similar questions are slowly accumulating. To anticipate the results, they give promise of greater flexibility (and challenge) in the shaping of behavior.

Berlyne (1950) presented the first controlled experiment on the assessment of novelty and curiosity as determinants of behavior. He introduced various novel stimuli into an exploratory box and measured the number of approaches and the time spent in the vicinity of the various stimuli, comparing these measures with time spent with more familiar objects and stimuli that were unchanged. With appropriate counterbalancing of elements across groups, he objectively determined that significantly more time was spent (level of significance from .02 to .006, depending upon the particular comparison) with novel stimuli than with familiar stimuli.

Montgomery (1951a, 1952a) started a large series of investigations that were stimulated initally by his interest in spontaneous alternation. In these particular studies, he allowed rats to wander about on elevated mazes and noticed that repetition of places was avoided rather than repetition of turns. Whether expressed as a tendency to avoid places where the rat had most recently been, or to go to places where the rat had least recently been, the behavioral effects are the same. In an additional study, Montgomery (1952b) collected additional data on the relative importance of exteroceptive stimuli in the determination of behavior. In this instance, he used two alley mazes that intersected at right angles. With this arrangement, he was able to isolate the place the rat went from the response made by the rat by changing the start box to the opposite end of the alley on successive trials. If a rat had gone to the right on the first trial, then a response to the right on the second trial, starting from the opposite end, would be a repetition of the response, but a change in the place (the rat would be going to the opposite end of the intersecting alley). By this method, he again demonstrated that the tendency to avoid a particular place was of greater significance in alternation behavior, affirming the importance of environmental stimuli as a determinant of behavior.

Glanzer(1953a, 1953b) approached essentially the same problem, but in a slightly different manner. Instead of isolating the place and response components in the same manner as Montgomery, he used a T maze with detachable end boxes. Following the first trial, the two ends were switched. It was determined in this manner that the rats would make the same response in order to go to the unexplored end. He discussed these results in terms of stimulus satiation and further corroborated the interpretation by showing that the effect of delay (time between trials) was a function of the place of delay. It will be recalled that the amount of alternation decreased with larger times between trials (Heathers, 1940). Glanzer, on the other hand, found that the amount of alternation is greater if the delay takes place in the end box than if it occurs away from the end box. Glanzer attributed this to the effect of the stimulus elements.

Walker and his collaborators (1955a) have confirmed the importance of stimulus elements in spontaneous alternation. They also used a maze consisting of two alleys intersecting at right angles. It was designed so that any alley could be blocked to form a conventional T-maze. By varying the start box and rotating the maze in the room they were able to assess the relative importance of the intramaze stimuli (stimulus), extramaze stimuli (place) and the turning movement (response) in spontaneous alternation. The general results were that the stimulus was the most important factor in alternation. Place was also important at a significant level, but the response was of little or no importance in spontaneous alternation.

Working on the basis of a "stimulus satiation" theory, Walker, et.al., (1955b) gave their rats a pre-exposure to the stimulus associated with a goal box (black or white), hypothesizing that this would result in the rat responding to the goal box of the shade that had not been "satiated." They found that pre-exposure in a separate box for as long as 30 minutes did not influence the response made by the rat when placed in the maze. Another condition used was to place the rat within one of the goal boxes for 45 seconds before the free trial. In neither case did they find that pre-exposure would influence the subsequent behavior of the rat when given a free trial. On these grounds, they concluded that the rat was alternating its "choice" on subsequent trials in the maze.

This particular finding is somewhat reminiscent of the difference in results obtained by Carr (1917) and Wingfield and Dennis (1934). Carr found several hundred trials necessary to train an alternation response when the trials were forced first to the left and then to the right, but Wingfield and Dennis report that if free trials are permitted, that is, if the experimenter capitalizes on spontaneous alternation and selectively reinforces it, the response sequence is very easily taught. It would appear here, also, that choice at the choice point is what is alternated. Fortunately, an alternative explanation is available. Although detention in a separate stimulus box or in a particular end box fails to influence a rat's subsequent response in a maze, it does appear that detention at the choice point may make a difference. Kivy, Earl, and Walker (1956) have specifically tested this hypothesis. They used a T-maze which was designed so that the arms could be closed by glass partitions. They placed the rats in the stem for 1, 15, or 30 minutes with the maze arms blocked and both the same shade, either black or white. After this pre-exposure they removed the partitions and changed one of the arms from black to white, or from white to black, so that one of the arms remained the pre-exposure color (presumably satiated) and the other was not. Under these conditions, they did not find alternation (response to the arm other than the one that remained the pre-exposure color) for the one minute pre-exposure group, but they did have significant results for the 15 and 30 minute pre-exposure conditions. With exposures of this duration, they obtained alternation on about 85 percent of the occasions which they considered to demonstrate the importance of exposure to the particular stimulus in the context of the choice point as being the essential factor in alternation.

It should be noted that the series of experiments conducted by Walker et al. are not entirely free of criticism. It will be recalled that in the first experiment, stimulus referred to the particular place within the maze, place referred to the orientation of the maze within the room, and response, of course, referred to the particular lucomotor behaviors of the animal. The important point to be made from this is that stimulus did refer to a place within the maze, a place that had attributes other than the stimulus color used for that portion of the maze. On the free trial following the satiation procedures, the rats found two places, neither of which had been previously explored. They had no particular reason for seeking (or avoiding) either one. By the same token, in the last experiment, they found two unexplored places (with equal valence on this count), one of which had been changed.

Dember (1956) devised the alternative hypothesis that the rats were simply responding to (exploring) the arm that had been changed. He modified the design so that the rats were given 15 minutes in the stem with the arms, one black and the other white, blocked by glass. He then changed one so both were the same color for the free trial. Seventeen of 20 rats went to the changed arm on the free trial. Dember concluded that the rats were reacting to the changed condition.

Walk (1960) replicated the experiment performed by Dember (1956) with dark and light-reared rats. He obtained the same results with both experimental groups as well as with a group of colony rats run in a pilot study. Another group of colony rats that were run on a free trial without having been pre-exposed to the conditions at the choice point showed no preponderance toward either arm of the maze. It would appear on this basis that the re-activity to stimulus change is not dependent upon learning.

At this point, it appears that this type of experiment has a high degree of reproducibility, but not all of the results are positive. Levine, Staats, and Frommer (1958), however, attempted to reproduce the experiment performed by Dember (1956). They used a Y maze with glass partitions and arms that could be changed in color. The remaining experimental conditions were the same as those reported by Dember. But, in this case, they found that the free trials were distributed about equally to the two arms. There was no indication of any sort of reaction to the changed arm. Following this, they switched to the procedure reported by Kivy et al. (1956). Here again they obtained negative results. It seemed to them that their findings were hard to reconcile with previous findings in the area.

Dember (1958) did not find it so difficult to produce data consistent with previous results. He noted that Levine, et al. (1958) had used a Y-maze, (significant at .001 level of confidence) but with only a three-minute exposure period rather than the 15 minutes used in the previous studies and in the study by Levine, et al.

Glanzer (1958) has continued to explore the relevance of stimulus satiation and attempted to test the importance of the exposure being made at the choice point as was the case in Walker, et al., (1955b) and Kivy et al., (1956). He placed rats in either one arm of a maze or permitted them to go to one arm of a maze and then detained them there for one minute. His design was counter-balanced with respect to arm colors. He found a significant number of the rats went to the opposite arm on a free trial.

Generally, some sort of reaction to visual stimulus elements seem to be very well established as a determinant of animal behavior, whether expressed in terms of avoiding stimuli previously experienced or seeking stimuli not yet encountered. Change of stimulation commands the attention of the animal. Both Dember and Millbrook (1956) and Montgomery (1953b) have shown that the amount of change from one stimulus situation to another is a determinant of the animal's behavior. In the case of Dember and Millbrook (1956) three shades were used in three arms, black, grey and white, in a design similar to the one used in Dember (1956). Twenty of 28 rats made the predicted response when the predictions were based on the assumption that the rats would react to the arm that was characterized by the greatest change between the exposure period and the free trial.

These results were significant at the .02 level of confidence by the sign test.

The experimental results suggest two related but separable mechanisms in the sensory control of behavior. The first, supported by the analysis of alternation behavior and the Dashiell maze studies, is simply an exploratory tendency that is elicited by a new environment. It would be a trifle anthropomorphic to say that creatures strive to organize or structure their environment, but it is quite accurate to say that they behave in such a manner that this could be accomplished. But this is incidental. The important point is that this constitutes one condition that commands the attention of the animal. The second mechanism is that of introducing some sort of change in the environment. This might involve introducing novel objects or painting the walls. As will be pointed out later, it may simply involve turning on a light. This mechanism has been amply demonstrated by the work of Berlyne and Dember and is further confirmed by an experiment by Williams and Kuchta (1957). They permitted two groups of rats to freely explore a symetrical Y maze. For the control group, the maze had two black arms and one white arm. The white arm was explored significantly less than the black arms. The experimental group initially had three black arms, but on the fourth day, a white arm was substituted for one of the black arms. The relevant comparison, then, was the amount of exploration of the white arm by the experimental group (for this represented a reaction to change) and the control group during the last three days of the experiment. As expected, the experimental group explored it significantly more (p. =.01) than the controls.

Environmental Complexity and Exploratory Behavior

Demer, Earl, and Paradise (1957) developed a theoretical model for predicting responsiveness to stimuli of differing complexity and performed an experiment that they felt confirmed the model. In brief, the model assumes that both the animals and the various aspects of the environment are characterized by some particular level of complexity. Secondly, an animals response to the environment (in terms of relative complexity) is a function of his own level of complexity. Given alternatives, relatively simple animals respond to relatively simple environments; complex animals respond to more complex environments. Third, the animal's level of complexity increases as a function of commerce with his environment (assuming that the complexity of the environment has been elected and is not too simple nor too complex). The upshot of the model is that if an organism is given alternative environments in terms of stimulus complexity, it is not possible to predict what level of complexity the animal will select initially, but it is possible to predict that any subsequent selection of a new environment will be in the form of choosing a more complex environment. To test the hypothesis, they developed two contiguous circular mazes, so that passage was available from one passage to the other. In the first experiment, one maze had vertical black stripes and the other, the less complex, horizontal black stripes. The rats were introduced into a start box that was adjacent to both. Twelve rats were introduced into this situation. Nine initially selected the more complex maze, while the remaining three went into the maze with horizontal stripes. In the second experiment, the more complex maze had horizontal stripes and the less complex maze was either plain white, or black. Eight rats were used in each contingency. Six of the eight went initially into the black alley and two of the eight went initially into the white alley for a total of eight in the simple and eight in the more complex. The support for their model came from the fact that of all the animals that changed from one alley to another, significantly more changed from relatively simple to complex mazes than the reverse. But, the incongruities of their data also strongly suggest that the matter is more complex than this. If a simple linear scale is used to represent the three degrees of maze complexity, three for the most complex (vertical stripes) and one representing the least complex (black or white, no stripes), then the mean complexity of the first sample (experiment 1) is 2.75 in contrast to a mean of 1.5 for the second sample (experiment 2). Further, the reviewer took the liberty of doing a chi square test of the significance of the difference for the black and white mazes and found this to be significant at the .05 level of confidence. It seems that in this experiment, as the one reported by Williams and Kuchta (see above), the rats show a definite tendency to avoid a white maze unit. In any event, the black and white mazes are not characterized by equal valences as was assumed in the experiment.

Fear Responses and Exploratory Behavior

Additional factors have been shown to be of considerable importance in the determination of exploratory behavior. This seems reasonable enough, for anything that can otherwise influence the behavior of an animal will, of necessity, alter the amount of exploration under appropriate conditions. The finest example of this is afforded by the traditional need-states. An animal that has gone without food or water (or both) for 24 or

48 hours is not likely to waste time sniffing every nook and cranny when there is food in the goal box. On the contrary (and especially after a few trials), they tend to go to the goal with great dispatch.

Another condition influencing the amount of exploratory behavior is that of fear. Barnett (1958a), in his studies on neophobia in rats, found that wild rats tend to avoid an unfamiliar object in familiar surroundings. This was also true of hooded rats to a minor degree, but patently false of the albinos. The intrusion of a new object into the home cages of wild rats completely disrupted the eating of some. One rat went four days without eating when the food was moved from a basket at the rear of the cage to an unfamiliar tin that was placed in the front of the cage.

Montgomery (1955) has also noted that fear can be elicited by a novel situation. He observed the approach-avoidance behavior and exploratory behavior of rats exposed to enclosed or elevated mazes and found that there was much more avoidance of elevated mazes than there was of enclosed mazes. He noted that the fear evoked decreased as a direct function of the time of direct exposure, and that there was some spontaneous recovery of fear, depending upon how much fear was previously extinguished. It might be noted that Barnett's rats, once they adapted to the novel stimulus, increased their food intake and movement within their cages. This suggests that the avoidance of some stimuli is due to the unfamiliarity of the stimuli. As the animal becomes familiar with the stimulus, the fear response is "extinguished," i.e., the stimulus no longer elicits an avoidance response.

Montgomery and Monkman (1955) have further explored the relationship between fear and exploratory behavior. They found that the amount of exploration of a situation decreased in proportion to the amount of fear (induced by shock) elicited by the situation. The decrement in exploration was not attributable to the incompatability of responses, for when they induced fear immediately before the rat was given access to the maze, there was no evidence of the typical decrement in exploration. Hence it appears that the decrement in exploratory behavior may be due to an avoidance of the fear-eliciting stimuli.

Berlyne (1950) observed that when placed into an exploratory box for the first time the rats would assume a rigid stance and would not move for several seconds. During a five minute exploration period the maximum time spent exploring novel objects was 39 seconds. After having investigated the objects placed in the alcove of the box the rats would typically retire to a corner or to one side of the box.

These results are highly consistent with results obtained when the effect of past experience were investigated. Ehrlich (1959, 1961) in the course of two experiments, investigated both the effects of early handling and free environments (environments that had a wide variety of stimuli available) on the subsequent amount of exploratory behavior in Y and Dashiell mazes as well as on the subsequent effectiveness of an audible click and light onset as a reinforcing stimulus for a lever response. She found that handling, which consisted of picking up the rats and stroking them ten times daily, resulted in more exploratory behavior in both types of mazes. Free environment rearing, on the other hand, tended to reduce the amount of exploration when compared to rats that had been reared in a restricted environment, but the difference was not statistically significant. In spite of the lack of statistical significance, it appears that the variable is important. The restricted environment rearing resulted in significantly more lever responses than free environment rearing, while in this case differences due to the amount of handling was near to, but failed to attain the usual 5 percent level of significance.

By way of general interpretation , it would appear that handling would function in such a manner to reduce the fear that might otherwise evoke an avoidance response. The rats, in a sense, grow to trust their masters. On the other hand, the fact that a rat will engage in more exploratory behavior when raised in a restricted environment somehow suggests the notion of wide-eyed innocence. Or, if emphasis is placed on their counter-parts that were raised in the enriched environment, familiarity breeds contempt. Zimbardo and Montgomery (1957) have conducted a similar experiment in respect to the effect of free-environment rearing on exploratory behavior. They found essentially the same results, that rats raised in a free environment engaged in less exploratory behavior than rats raised in a normal laboratory environment.

It is presently impossible to determine to what extent differences in the amount of exploratory behavior can be attributed to fear. While this seems to fit quite well the data provided by Barnett (1958a, 1958b) there are also differences among laboratory strains. Carr and Williams (1957) tested the exploratory tendencies of three highly inbred strains, black, hooded and albino. The hooded rats spent significantly more time in exploration (and less time sitting) during a 10 minute test period in a white Y maze for the five days of the experiment. Where there are individual differences and strain differences, one can be fairly certain that there will be differences among species. To date there has been no systematic comparison of species, for it is difficult, if not impossible, to make valid comparisons. But, Ehrlich and Burns (1958) have at least recorded exploratory behavior on the part of the members of another species, the black-footed ferret.

Exploratory Behavior and Activity

It is necessary to consider the relationship between exploratory behavior and general activity for two reasons. In the first place, both are measured by some sort of quantification of the amount of behavior. Exploratory behavior, especially in a situation such as that presented by a Dashiell maze, is evidenced by the movement of an animal. But at the same time, activity, such as is presumed to result as an increase in drive, is often measured in precisely the same manner. It may be recalled that the Dashiell maze, which has received such extensive use in studies of exploratory behavior, was first developed for the purpose of measuring this type of activity (Dashiell, 1925). Secondly, and this is of considerable importance, the effect of increase in drive (or the effects of various conditions of deprivation) on exploratory behavior is of interest.

At this point one may question whether exploratory behavior can in fact be distinguished from activity. It must be confessed that in the above illustration the two seem completely confounded. But it is quite apparent, especially in light of the studies resulting in the analysis of spontaneous alternation, that the exploratory mechanisms result in behavior that is ordered in terms of the available stimuli. Secondly, the amount of exploratory activity decreases as a function of the amount of time spent in exploration of the environment. But the question remains; what is the relationship between drive and exploratory behavior?

The first experimental results bearing of this question showed a decrease in the amount of exploratory behavior. Montgomery (1953b) compared the number of units of a Y maze that were traversed by sated rats and rats that had been deprived of food or water. The sated rats, in this instance, engaged in more exploratory behavior. But the conclusion should be held tentatively, for the studies to be reported next should suffice to reveal that the matter is more complex.

Adlerstein and Fehrer (1955), using a more complex maze, found that under 23 hours of food deprevation animals explored from 50 percent to 75 percent more than did their satiated counterparts. But Zimbardo and Montgomery (1957a) also used a complex maze and again found a relationship in the opposite direction, hunger and thirst reducing the amount of exploration. This could be in part because they used a checkerboard maze of nine squares, each square having food or water, depending upon the relevant need. In this situation there is a conflict in responses. Both thirsty and hungry rats would explore several minutes before drinking or eating.

Clickman and Jensen (1961) used a Y maze and 22 or 46 hours of food and/or water deprivation and found that the amount of exploration after 46 hours of food deprivation to be greater than that after 22 hours of food deprivation. There was no evidence of drive summation over hunger and thirst deprivation. After 22 hours of water deprivation rats explored more than they did after 22 hours of food deprivation, but not significantly more than after 22 hours food and water deprivation. Stackhouse, et al.,

(1960) found that exploratory behavior was related to need if the open field contained food. Finally, Hall, Low and Hanford (1960) found that satiated rats were just as active in a Dashiell checkerboard maze as those that had been deprived of food or water. Tests were made of the number of units traversed for ten minutes a day over five days.

It is possible to reconcile much of the conflict in results in terms of the conditions preceding the testing of the animals. Exploratory behavior is very closely related to generalized activity to the extent that in open field tests both employ some sort of measure related to the amount of movement, such as the number of units traversed.

Reid and Finger (1955) had reported that the activity of rats increased as a function of adjustment to a 23-hour schedule of deprivation, but they later noted (Finger, Reid, and Weasner, 1957) that much of the increase they had reported was probably due to their experimental procedure. In the earlier experiment they had placed the rats in an activity wheel just prior to the time of feeding and hence activity-when-hungry might have been reinforced. This factor was controlled in the later experiment in which they recorded the activity of rats in an activity wheel every other day under 21 hours of food deprivation, but only half the animals were returned immediately to the home cage for feeding, the others remaining in a neutral chamber for an hour before being returned and fed. Under these conditions, the group fed immediately after activity exceeded the delay group in activity by about 28 percent, but both groups showed some increase for the following thirty days. The same research workers in a related experiment (1960) used essentially the same design, but used three hours of deprivation in order to further explore the relationship between deprivation and activity and found the same effect, the group transmitted immediately to the home where they were fed showed more activity than the delayed group. Following this phase, they subjected the rats to deprivation from food for 27, 51 and 75 hours. The delay group showed an increase in activity under these conditions of deprivation, the amount of activity being monotonically related to the amount of deprivation, but the group that had been immediately returned to the home cage (hence reinforced for being active) showed even greater activity, indicating that it is possible to inadvertently train rats to be active in designs where they are tested under 23 hours of deprivation and then returned to the home cage for feeding. Birch, Burnstein and Clark (1958) have also demonstrated a relationship between maintenance schedule and activity. After having maintained 39 rats on a 22 hour food-deprivation schedule for five weeks, they recorded the number of food-trough responses throughout 46 hours of food deprivation. The number of trough responses was lowest immediately after feeding and maintained at a low level until about the fifteenth hour,at which time there was a fairly sharp increase in the rate of response to quite a high rate at the normal feeding time. There was a further increase to about 25 hours of deprivation,after which the rate of response dropped to a low level. Once again, as the normal feeding time arrived there was an increase in the number of trough responses. In addition to recording the number of trough responses, they recorded alley speeds for groups of rats that had experienced 15, 22, 25 and 37 hours of food deprivation. Using two pellets of food in the goal box, they analyzed the results for the first trial and trials 2-4 separately. They found the results of the reinforced trials 2-4 corresponded closely with the results of the food-trough response data. The speed was slowest at fifteen hours of deprivation, much faster at 22 hours, fastest at 25 hours and the speed decreased to a fairly low level following 37 hours of deprivation. The first trial results were quite different, being about the same for all levels of deprivation. The authors of this study interpret it to support the position that drive and hence activity is controlled by both external and internal sources of stimulation. Two factors emerge from the study that are of particular interest in respect to exploratory behavior. The first is the fact that the hunger groups were not differentiated on the first trial, the trial that most closely resembles the exploratory situation. The second point, of course, is that the introduction of food into the situation modified the relevance of the conditions of deprivation. Unfortunately, the results are reported only in terms of speed through the alley maze. Speed, while related to the amount of exploratory behavior, is not a very good index.

In addition to maintenance schedules and external stimuli, the normal patterns that characterize the behavior of most species have been shown to be of importance in the case of rats. Bare (1959) trained six groups of rats to secure food by lever pressing. Following this he placed the rats into the test apparatus following 2, 4, 8, 12, 18 and 24 hours of food deprivation, the deprivation having been scheduled so that the test session started at 7 p.m. for all groups. He observed a very strong cyclical pattern in food consumption that corresponded to the day-night cycle. The strength of this pattern was strong enough to mask the effect of deprivation if the deprivation terminated at different periods of the day.

In addition to the effect of feeding schedule on activity, increments in activity that have been observed in several studies have been attributed to a lowering of the threshold of extrinsic stimulation. Campbell and Sheffield (1953) studied the random activity of rats that were maintained in a constant stimulus environment. They observed little increase in activity due solely to deprivation, but noticed that there was a great increase in activity during a five-minute period marked by great environmental stimulation. The importance of stimulation in producing activity seems clear in this study.

Teghtsoonian and Campbell (1960) have pursued this line of investigation further. They housed two groups of eight rats individually in stabilmeter cages designed to record the activity of the rat within the cage. All rats were deprived of food until they died. One group was placed in sound-isolated buildings, ventilated by an air conditioner which provided a masking sound. The other group was maintained in cages placed in the laboratory. The experimental group, maintained in the laboratory, showed an increase in the amount of activity by the sixth day of the experiment to a level 400 percent greater than the amount evidenced during an adaptation period. In contrast to this, the control animals in the isolated cages increased their activity to a level only 70 percent greater than the level of activity that characterized them during their adaptation period. So both groups became more active as a function of deprivation, but by far the greater increase in activity was attributable to greater responsiveness to stimulation.

Unfortunately, these considerations fail to reconcile all of the conflict in the literature, Hall (1956) also investigated the relationship between hunger drive and external stimulation with quite different results. Using the amount of activity in an activity wheel for his dependent variable, he varied both the amount of deprivation and the amount of stimulation and obtained results that conflict with those just cited. He tested his rats in the dark and subjected them to noises and flashing lights as well as deprivation in a counter-balanced design. He found most of the activity attributable to the time of deprivation and only a little to external stimulation. Of course,there is a gross difference between this type of stimulation and the type of stimulation normally available in the laboratory. It is conceivable that there would be differential responsiveness to the two. It is also possible that the usual nature of the stimulation, which must have been different from any the rats had ever experienced, may have produced inhibitions in them, if not fear responses.

Bolles (1959) has concerned himself with the conflicting results reported in the literature and has done much to clarify the cause of the conflict, even if he has not resolved it. He tested 51 rats in six situations that yielded 20 different performance scores. Each rat was subjected to four different deprivation conditions, high and low hunger distributed over high and low thirst. He found that the performance measures were not related in any consistent way to either intensity or kind or deprivation, but rather that the facilitory effect of a particular deprivation seemed very selective as to the specific performance measure and the particular rat involved. Individual differences seemed to him to be very situational, showing little generality across situations or conditions of deprivation. Bolles has argued from these and other results against the usefulness of the drive concept in learning. However, Bolles results should be accepted with hesitancy, for to some extent they may be a product of procedure. While no reliability coefficients are given for his measures, the data on factor communalities suggests that the measures have low reliability. While Bolles contends that he has contributed data to show that activity is highly specific to particular situations and conditions of deprivation, it may well be that his specificity is nothing more than what the psychometric specialist would call error of measurement. Until the study has been repeated with greater attention being given to the measurement procedures, the latter conclusion is the most likely.

DeLorge and Bolles (1961) conducted an additional investigation using multiple measures of behavior. In this case they placed the rats in a box equipped with windows and classified the behavior of the rats as locomotion, window peeking, manipulating, investigating, peering, grooming and freezing. The rats had had free access to food prior to testing and deprivation, and the behavior was recorded at five second intervals. There was little relationship between measures except the amount imposed by the experimental design. The most significant effect was that the total amount of grooming was negatively related to the amount of deprivation. As the rat spent less time in grooming, it necessarily resulted in an increase in the number of observations on one or more of the other measures since the experimenters made a total of 120 observations during each experimental session. The variable which showed the greatest amount of increase, as the grooming behavior declined, was the one classified as investigating, which increased steadily in frequency over the 48-hour span during which the rats were observed.

The internal conditions related to activity which have been considered up to this point are those produced by food and water deprivation. The ease with which the variables in these studies can be controlled make them a first choice for the experimentalist. Ofher attempts have also been made to control intraorganic variables, and one approach which has been investigated is that of restricting the activity and relating such restrictions to the subsequent level of activity. Such a study is modeled in a general way after studies of food and water deprivation. Some of the studies conducted along these lines have used Murphy's assumption (1947) of a general activity drive, but just what is being deprived is not at all clear. If the movements of an animal are restricted, should one say that it is being deprived of movement, or is it deprivation of muscle sensation and skin contact sensations? Control of movement also results in visual deprivation, for movement brings about changes in the visual world. Studies of activity deprivation are obscure in what they involve.

Hill (1956) used 40 male albino rats, which had been confined 0, 5, 24 or 46.5 hours in very small cages that restricted their activity and found that the amount of running in activity wheels following confinement was a monotonic increasing function of the hours of prior confinement. He reported that the relationship was almost linear.

In a closely related study, Hill (1961) maintained 16 rats for 24 hours with continual access to food and water in small sheet metal cages, small wire cages, large wire cages or small wire cages with activity wheels attached. He then gave them 100 trials in a T maze, one side of which terminated in a small box while the other side led to a Dashiell maze, which by its structure permits the rat to engage in extensive activity. All groups showed an increasing preference for the Dashiell maze and the activity therein varied inversely with the degree of confinement previously experienced. The effect of the amount of confinement was significant at the .05 level of confidence, but only the animals confined in the wheel cage showed significantly less activity than the other groups in the Dashiell maze. Running speed was not influenced by the amount of deprivation of activity. The latter, also was not related to the frequency with which they chose the Dashiell maze rather than the more confining alternative in the T-maze. In a subsequent experiment in which previously confined rats could choose to enter either a cage or activity wheel, the amount of previous deprivation was unrelated to the frequency of one choice over the other.

An earlier study by Siegel (1946) had failed to find a relationship between movement deprivation and subsequent activity. The reason for this apparently inconsistent result may be that Siegel placed his rats, after confinement, in a chamber with uniform light equipped with a masking noise. The environment was similar to that provided by Campbell and Sheffield (1953) who found little increase in activity as a result of food deprivation so long as the rats were maintained in a constant stimulus environment.

Montgomery (1953c), in his studies on exploratory drive, obtained results that conflict with the results obtained by Hill (1956, 1961). The major difference between the two sets of studies is the duration of the confinement in the small cages, Montgomery confining his animals for a period of eight days as contrasted to the 24 hours confinement used by Hill (1961). Montgomery failed to find differences in the amount of exploration of a Y-maze between his experimental animals and control animals that had access to an activity wheel.

An overview of the research dealing with broad internal and external conditions related to amount of activity shown by an organism brings out an interesting trend in thought concerning the matter. While the early studies explored the role of internal conditions in increasing or decreasing the general level of activity, the more recent studies have focused on the importance of external factors and their interaction with internal factors. It seems well established that the general level of activity is not dependent on conditions internal to the organism alone, but is also highly dependent on environmental conditions.

The relationship between various need states and exploratory behavior, on the other hand, is not well established. Does a high need state enhance or hinder curiosity? The maze studies in which food is used to reinforce hungry animals demonstrate, of course, that need-relevant stimuli result in responses incompatible with exploratory tendencies. It has been maintained for centuries that one cannot philosophize on an empty stomach, but the proposition has never received experimental confirmation.

Exploratory Behavior and Reinforcement

Considerable experimental effort has been expended on the problem of exploratory behavior, both in the demonstration of the phenomenon and in explicating the determinants of the behavior. The reasons for this are both numerous and complex, but of primary importance is the development of a general dissatisfaction with S-R drive reduction theory. This type of dissatisfaction was expressed by Harlow (1953) when he commented on the overconcern with tissue-tension types of motivation.

The latent learning studies, and the difficulties in which they eventually floundered was taken by some to reflect the fundamental soundness of the drive-reduction reinforcement position, but such a conclusion was not justified. Theories are not sustained merely through a lack of evidence that runs contrary to them. A sound scientific theory must be such that it is conceivable that evidence could be found which is not in accordance with it. The outcome of the latent-learning type of experiment was to raise the possibility that the nature of drive-reduction reinforcement theory might be such that under no conditions could clear-cut negative instances possibly occur. Whatever precautions the latent-learning experimentalists would take to eliminate the operation of drives and reinforcements, it was always reasonable to hypothesize that some unfamiliar drive was operating, and in some way being reduced by the operation of hidden secondary reinforcements. The problem was that drive-reduction reinforcement interpretations of learning suffered from being too good, too all inclusive and too incapable of being tested in conclusive experiments which could conceivably produce negative findings. Any imaginable experiment, whatever the findings, could still be interpreted in terms of a drive reduction viewpoint. The constructs, in brief, were no longer under experimental control. It will be recalled that the distinction between two uses of reinforcement was emphasized in an earlier chapter. On the one hand, it is used as an explanatory construct (outside of the laboratory), and on the other hand, as a scientific term designating an experimental procedure with specifiable outcomes.

As a consequence of this theoretical deadlock psychologists began to take a more empirical look at the behavior of organisms placed in relatively novel surroundings and exercised considerable caution in taking theoretical positions which might order and organize the data collected. The result of this empirical orientation has been the collection of data pertaining to learning in situations that involve, generally, exploratory behavior. MacCorquodale and Meehl (1951) commented several years ago on the elimination of cul entries in a multiple maze even though food was not provided in the end box. Since that time, studies have been specifically designed to see whether or not learning will occur in a situation in which only the amount of exploratory behavior is permitted to vary. The results are generally positive. Response learning was demonstrated by Montgomery (1954). He used a Y-maze with a right angle at the end of the arms that concealed the end of the maze from the rat at the choice point. He attached a Dashiell maze to one arm while the other arm simply terminated. When the rat entered the Dashiell maze, it was permitted one minute of free exploration. Following an "incorrect" response into the blind alley, the rat was detained for 15 seconds. Under these conditions the rats showed an increasing tendency to choose the arm leading to the Dashiell maze. Montgomery and Segall (1955) demonstrated discrimination learning with a similar experimental procedure. In this case they used a small T-maze to which black or white arms could be attached, the shades being reversible with respect to side. For half of the animals, the black arm of the maze led to a Dashiell maze and for the remaining animals the white arm led to the Dashiell maze. The rats learned the black-white discrimination in this situation; the results showed an increasing number of responses to the arm leading to the Dashiell maze. At the end of the learning trials, the rats chose the arm leading to the Dashiell maze on 75 percent of the trials.

This is in some respects similar to the experiment reported by Thompson and Solomon (1954) in spite of the fact that their trials did not result in exploratory behavior. They used two stimulus cards, one with vertical stripes and the other with an inverted isosceles triangle. The patterns were equal in respect to the amount of white and black. They presented two groups of rats, experimental and control, with the first stimulus card and recorded the amount of time the animals spent sniffing the stimulus, the time they spent in the stimulus box, and the time the animal spent looking at (in the direction of) the stimulus card during a ten minute trial. Following a two minute rest period, the procedure was repeated, but this time the control group had the same stimulus card and the experimental group received the new stimulus card. A significantly greater amount of response to the new card by the experimental group was evidence that the patterns had been discriminated.

Two experiments by Myers and Miller (1954) claim to provide evidence of learning motivated by "exploration." In both of these experiments an experimental box was used which Miller had previously used for the study of avoidance learning. The box was divided into two compartments, one black and one white, and the partition included a door through which a rat could pass. In the first of the Myers and Miller experiments, hungry rats were started in the white compartment and learned to pass through the door to the black compartment. Four groups of rats were given 70, 30, 10 and 0 reinforced trials. It is important to note that the last of these four groups did not have the opportunity of learning to run from one compartment to the other. In a subsequent learning series all of the rats, now satiated and nonrewarded, were required to learn the response of pressing a bar to open the door to the goal compartment. All four groups learned the task equally well, including the group that had had no previous learning experience in the box. In a second experiment these same workers showed that rats satiated for food and water would learn to gain access to the other compartment even though food and water were absent. They also showed that learning was contingent upon the animals either entering or seeing the second compartment and those that were not allowed to see or enter it did not learn. In addition, animals trained with massed, rather than spaced practice did not learn.

Berlyne and Slater (1957) demonstrated that rats will learn to select a path leading to spacious goal boxes or to a box containing complex stimuli rather than to blind alleys or empty goal boxes.

Chapman and Levy (1957) used running time as the dependent variable when running female rats down a straight alley to an end box providing novel stimuli such as the walls and floor covered with rough masonite or painted in black and white stripes. The reinforcing effect of novel stimuli was demonstrated both by a decrease in latency and in resistance to extinction. Incidentally, it was found that food reinforcement decreased the value of the novel stimuli as reinforcing stimuli.

Miles (1958) reared kittens separately from the time of weaning at three weeks, until eight weeks of age. Observation during feeding established the fact that the kittens handled neither the food dish nor the food; manipulation was limited to movements involved in eating. The latter ruled out the possibility of manipulation having become associated with primary drive-reduction. He then exposed the animals to a sequence of learning situations. He gave the kittens 25 trials daily in a Y-maze, one arm containing nothing, while the other contained either a rubber ball, a small box or a torn towel and continued the procedure until they reached a criterion of 18 of 20 responses to the variable stimulus side. After reaching criterion, the kittens were given 80 extinction trials, during the course of which the distribution of responses dropped to 50 percent to either side. He then arranged the previously unreinforced arm with an escape hatch through which the kitten could jump out for a period of free exploration of the room. The other arm, previously positive, contained the kitten's empty food dish. This should have been a difficult learning situation since it involved a reversal of previous learning. Despite this fact, all kittens learned again to the criterion of 18 to 20 trials to select the arm leading to the escape hatch. Finally, he gave the animals a discrimination problem while they had been deprived of food for four hours. For half of the animals, a 100 watt bulb indicated the alley leading to an escape hatch. Finally, he gave the animals a discrimination problem while they had been deprived of food for four hours. For half of the animals, a 100-watt bulb indicate the alley leading to an escape hatch. For the remaining animals, it led to an alley that was baited with a ball of food. The incorrect alley, one that simply terminated, was designated by a 15-watt light. All of the kittens learned the discrimination and learned to the same criterion, but the food rewarded animals reached criterion more rapidly (with fewer errors) than the animals that were rewarded with escape from the maze.

Butler (1954) directly investigated the reinforcement value of various stimulus situations for rhesus monkeys. He equipped a box, that could contain a monkey, with a window through which the monkey could explore the environment outside of the box. The window was arranged so that it was normally shut, but could be opened by the monkey at intervals by pushing on it. The number of responses during the testing sessions varied with the stimulus conditions provided outside of the experimental box. The greatest number of responses occurred when there was another monkey outside of the experimental box. An electric train was also found to have high reinforcement value, while inaccessible food and the standard situation (nothing available in the room) yielded fewer responses, the former producing more than the latter. It would appear, on the basis of these results, that responsiveness to complex stimulus situations is at least partially independent of the reinforcement history of the animal, for if it were not, one would expect a higher response rate to the food than to the electric train. The secondary reinforcing value of the sight of food would be greater, having been paired with drive reduction, than the sight of the electric train.

Mason and Harlow (1959) investigated the initial responsiveness of infant rhesus monkeys to various stimulus characteristics in an effort to control more directly for the differences in incentive value due to the reinforcement history of the animal. They provided their subjects with banana cubes, colored banana cubes and mixed foods and observed the responses to each. As measured by the number of hand-to-mouth contacts, the number of displacements from the food dish, and the number of the incentives eaten the infant monkeys showed greater responsiveness to the colored banana cubes and the mixed foods than they did to the unadulterated banana cubes. These results support the contention that differential reinforcement values need not be due to the association of the stimulus with "primary drive-reduction," but fail, of course, to show that the association with drive reduction is not sufficient for the purpose of establishing differences in reinforcement value. Both factors seem to be operating in many situations.

The incentive value of a stimulus condition seems directly related to the efficacy of the stimulus as a reinforcer. Butler, (1953), demonstrated that rhesus monkeys will learn a yellow-green discrimination in order to look out of a window to a more varied environment. His group of five monkeys readily learned the discrimination and continued to perform at a high level with no other incentive conditions present. There was in this instance no evidence of satiation as the monkeys continued to perform at a high level over numerous trials.

Moon (1961) conducted a similar study, but employed a bar-pressing response and a shorter inter-response interval than did Butler. Under these conditions, he found that there was an intra-trial decrement in the number of responses made by his subjects, but that there was no inter-trial decrement. His monkeys spent 50 to 60 percent of their time looking out of their cages.

Using rats to investigate the reinforcing effect of novel stimuli, Jencks and Porter (1960) adequately demonstrated the importance of the variable. They maintained their rats on dry food and provided them with moistened food once a day. Under these conditions, the rats decreased their consumption of dry food and would consume more of the moistened food, which, during the course of the experiment, was available also at the conclusion of the experimental trials. They divided their subjects into three groups, a food-rewarded, a novel-stimulus rewarded and a control group that was simply picked up at the termination of the trial. The rats were given 12 trials, one trial daily, on a six-unit multiple elevated T maze. The novelty group ran to a different stimulus on each trial, i.e., a table carpeted with cotton on the first trial, a blast of air from a fan on the fifth trial, a perfume stick on the seventh, nails strewn on the table for the tenth and an empty table on the twelfth and final trial. Both experimental groups made fewer errors (cul entries) during the course of the experiment and both made significantly fewer errors than the control group, but they did not differ significantly in their performance when compared to each other. The same results were obtained in the measure of running time.

The evidence that has accumulated supports the contention that a fairly important (potentially, very important) source of reinforcement has been traditionally overlooked. Much the same as was revealed in the review of consumables, it appears that any stimulus that can command the attention of an animal can serve to reinforce learning. This generalization will obviously have some limitations imposed upon it as additional data accumulate, but at the present time it seems quite important to emphasize the importance of sensory mechanisms. One very promising approach in those instances in which the responsiveness of the subject can be quantified has been taken by Premack (1959). The relevant stimulus variables are disregarded, essentially, in preference for a measure of response. His major thesis, which is empirically founded, is that any stimulus that elicits a higher rate of response than the response which is to be reinforced will serve to reinforce that response. In order to test this hypothesis, which will be clarified by an example, Premack (1959) gave a group of children access to a pinball machine and a candy dispensing machine to determine the operant rates of response. During the second test, half of those that had a higher rate on the pinball machine were placed in a situation where the operation of the pinball machine was contingent upon the operation of the candy dispensing machine. To complement this arrangement, the operation of the candy machine was contingent upon the operation of the pinball machine for half of the remaining subjects. This phase of the experiment was conducted three or four days after the first phase with the remaining subjects used as controls. Of the subjects that showed a higher operant rate on the pinball machine, the ones that had the operation of the pinball machine contingent upon the operation of the candy machine showed a greater increase in the number of operations of the candy machine than did those that had the reverse situation, the operation of the candy machine contingent upon the operation of the pinball machine. The results from the subjects with a high operant rate on the candy machine were complementary, showing an increase in the rate of response to operation on the pinball machine for those for whom the operation of the candy machine was contingent upon this response.

Kagan and Berkun (1954) showed essentially the same type of results with rats. The operant rate of running in an activity wheel is greater than the operant rate for a lever response for rats and they successfully used the former to reinforce the latter as measured by rate of response. In their experiment, the lever was rigged to release the brake that was applied to the activity wheel, and, on a periodic schedule, the experimental animals showed a significantly higher rate of response to the lever than the control animals. The promise offered by this sort of an approach is further illustrated by an experiment conducted by Hendry and Rasche (1961). They had noticed that thirsty rats would stand beside an air-stream and lick at it, much as though they were drinking it. The air-stream, then, was a stimulus event that elicited a response on the part of the animal. They subsequently learned that when the air-stream was used as a reinforcer, the alley speed of the rats showed typical growth and extinction curves.

A final provocative note is sounded by Havelka (1956) in an article on the problem-seeking behavior of rats. Two different experimental boxes were used for this experiment. During the first phase of the experiment, food was presented in a constant position during successive trials on one side of the box, but was moved from place to place on the other side of the box (see Fig. 3.1d). Throughout the course of the experiment the responses of the rats were distributed equally to the two sides of the box. But when the experimenter examined the behavior of the individual subjects, he found that there were three distinguishable groups that were very stable in their pattern of behavior (correlation was .80 for behavior during the first 30 and last 30 trials). One group, about a third of the total group, rather consistently went to the side in which the food was constantly located. Another third alternated sides. The last group selected uncertainty, variability, or whatever else might be found in the situation. They gave evidence of a decided preference for the problem situation. During the second phase it was necessary for the 'problem-seekers' to bypass the fixed goal and traverse a greater distance in order to gain the variable goal (see Fig. 3.1e). This they did very consistently. Not until the position of the food was restricted, to a fixed position on the distant goal, did they show a preference for the closer goal. Could it possibly be that one problem might serve as a reinforcing stimulus for the solution of another? One is reminded by the conclusion drawn by Thompson and Solomon: "It is assumed, probably falsely, that S finds no joy in doing the difficult or unusual...(1954, p. 106)."

Reinforcing Properties of Light

The fact that photo-reinforcement is reserved for a separate section is in no way intended to suggest that the mechanisms involved are to be conceptually distinguished from those in the preceding sections. Quite the contrary, it is simply a matter of expedience. There is a larger number of studies that do, in a sense, stand alone, but their relationship to exploration and curiosity will be evident.

The aversive effect of strong light has long been established by those who have worked with laboratory rats, and is a familiar topic to those that have left a theater on a bright sun-lit day. Flynn and Jerome (1952), for instance, used it as a drive stimulus in training a response. Caldwell and Womark (1953) found a trend in both errors and running speed that supported a drive interpretation of strong light when they compared the performance of mice that were run to light and dark goal boxes from a brightly lighted alley in a T maze. Kaplan (1957) has attempted to develop a theory relating escape functions to the intensity of light and has generated theoretical curves of this relationship. In view of these findings, it is somewhat surprising to find that light should have adient as well as abient properties.

The first reports of the use of light as a reinforcing stimulus occurred just ten years ago (Henderson, 1953; Kish, 1953). Kish habituated a group of mice to total darkness in a modified Skinner box and obtained an operant level of response. The animals, which had free access to food and water except for the time in the experimental box, were divided into two groups, the experimental group experiencing the onset of illumination contingent upon the lever response while the control group remained in darkness. At the end of a 25-minute trial, the experimental group was making significantly more lever responses than the control group. To test the hypothesis that the increase in response rate may have been a function of an increase in activity due to the photic stimulation, another group was also subjected to light stimulation, but with the onset of the light being uncorrelated with the behavior of the rat. Under these conditions, the light did not show a significant effect. It might appear, then, that the increase in response can be attributed to the increase in activity. But the fact that there is more involved in the effect is demonstrated by the two following experiments.

In a similar experiment with a different species, Moon and Lodahl (1956) used 20 rhesus monkeys in a situation where a lever response would result in a change in illumination, either an increase or a decrease. A control group of 11 additional monkeys had no stimulus change contingent upon the lever response. In comparison with the control group, both experimental groups showed significantly more lever responses, demonstrating the reinforcing value of not only photic stimulation, but a change in illumination for rhesus monkeys. A decrease in stimulation would not have the properties of a drive stimulus.

An experiment to control for the increase in activity that might result from the increase in light stimulation was undertaken by Kling, Horowitz and Delhagen (1956). Twelve pairs of rats were given 20 minute sessions in darkened boxes which had an immovable bar attached to one wall. After adaptation, both animals in each pair were exposed to illumination in the interior of the same boxes. The onset of illumination was correlated with the behavior of the experimental animal, and was arranged so as to be contingent upon a contact with the bar; that is to say, when the experimental member of a pair touched a bar the light went on not only in the box of the experimental rat, but also in the box of the control rat. Thus, experimental and control rats were exposed equally to photo-stimulation. The experimental group showed significantly more contact with the bar than the control group. Kling _et al_., suggested that the reinforcing effect, which was of relatively low efficacy, might be due to the novelty of the stimulus change.

Robinson (1959) used rats that had been maintained under normal lighting conditions to determine the reinforcing effect of a change in photic stimulation. One group of rats had the onset of a light contingent upon a lever response, while the other had the offset or termination of a light contingent upon a lever response. In comparison with the operant levels of response, the group receiving the onset of light showed a significant increase in the number of responses, whereas the group that experienced the termination of a light showed no effect in the number of responses. The amount of illumination during the operant session was also unrelated to the reinforcing effect of the light onset, as measured by the number of responses made by the animal.

Many investigators have studied the conditions influencing the efficacy of light onset as a reinforcing stimulus. Some of these studies have attempted to determine the effect of an irrelevant drive on the phenomenon. Davis (1958) maintained a colony of rats on a 23 hour food deprivation schedule for seven days and then arranged his experimental sessions so that the experimental groups had eaten 0, 2 or 23 1/2 hours before the experimental sessions. The first three days were used for the determination of the operant level of lever responses. This phase was marked by a decline in the number of responses during the successive days, but the number of responses made was an increasing monotonic function of the hours of deprivation. Following the determination of the operant level for the three experimental groups, a variac illumination screen was arranged so that a weak light onset was contingent upon a lever response. The response level during the fourth day, the first experimental day, was strongly related to the amount of deprivation, the greater amounts of deprivation resulting in greater increase in the number of responses. The fifth and sixth experimental days were again marked by a decline in the number of responses made by all of the groups, but at the termination of the experiment all groups performed at a higher level than they had during the last.operant session.

Another study of the effect of an irrelevant drive on photo reinforcement is that by Clayton (1958) who maintained 22 male albino rats on a 23 hour water deprivation schedule for 14 days before determining the operant level of lever response. Following this, he arranged a 7-watt light with a half-second duration so that it would follow a pressing of the lever in the experimental box. Analyzing the difference between the number of responses made during the operant and experimental sessions, he found that the water deprived rats made a significantly greater increase in the number of responses than did the non-deprived rats. In an analysis of the variance of the difference scores, the interaction between time of deprivation and reinforcement was significant at the .05 level of confidence. These results are in accord with those obtained by Davis (1958) in relation to food deprivation, but they are not in accord with the findings of Hurwitz and De (1958). Using groups of rats that had experienced 6, 7, 12, 17 and 22 hours of food deprivation, Hurwitz and De (1958) used a light to reinforce a lever response, but failed to find a clear-cut relationship between the amount of food deprivation and the number of responses made by the subjects.

In a related study (Hurwitz, 1960b), four rats underwent training.

In phase I the rats were adapted to a Skinner box, a lever pressing situation, and an operant rate of responding was determined. In phase II each S was given six half-hour trials distributed over six days, in which a press of the lever resulted in light onset. In phase III a lever press turned on the light and also delivered a pellet of food. In phase IV the food reinforcement was withheld, but a lever-press continued to result in light onset. Under seven daily half-hour sessions of this latter condition there was no decline observed in the lever pressing response though the response to the food trough was extinguished.

While considering drive level in relation to reinforcement, some mention must be made of another study by Hurwitz (1960c). Although this study was not undertaken within the framework of a theory calling for the concept of drive, it can be considered as a typical study of the effect of drive, as measured by deprivation time, on operant conditioning. The deprivation in this case involved darkness, a condition of sensory deprivation, which some would consider to be a condition which would increase the strength of a curiosity drive, a stimulation need, or whatever one likes to call it. Hurwitz used two groups of five hooded rats and gave them seven half-hour operant sessions in the experimental box. For one group, the box was illuminated and for the other, the box was dark. During these sessions the rats introduced into the dark box made about twice as many responses (p. = .10) than the rats tested in the illuminated box. Following this, the rats were all introduced into a dark box and the apparatus was arranged so that a bar press would result in light-onset. Both groups made significantly more responses than during operant conditions, but the rats that had been previously introduced into a darkened box also made significantly more responses than the group that had experienced the box only under illuminated conditions. Throughout the 14 experimental sessions, the light onset was for the duration of the bar contact, but while it is possible that the two groups would be differentiated in terms of mean duration of bar contact, the relevant data are not reported. The study suggests that deprivation from light has the effect of enhancing the reinforcing properties of light onset.

Crowder and Crowder (1961) have explored the relationship between duration of light onset and response tendency. They reinforced ten lever responses with a light of 1/3, 1, 3 or 9 seconds duration. An analysis of the number of responses made during a twenty minute extinction period yielded means of 11.4, 14.2, 15.8 and 13.1 respectively. An F-test for the significance of the differences gave negative results. They provide no data on the responsivity of the rats to the lever with the divers light durations used as reinforcing stimuli, so a measure of extinction is the only criterion of learning in this experiment.

To test the hypothesis that a change or variation in stimulation is a crucial factor in the reinforcing effect of light onset, Morris, Crowder and Crowder (1961) designed an experiment to test the effect of the amount of stimulus change. They used three groups consisting of 16 rats and reinforced them either with a bright light, a dim light, or a bright and dim light on alternate trials for 10 acquisition trials. They followed the acquisition trials with a 20 minute extinction period during which the number of responses was recorded. They did not find a relationship between the experimental conditions and the number of responses emitted during the extinction period. This study is open to criticism since training was brief and can hardly have produced much change in response rate. It is not surprising that the extinction series yielded negative results for there could have been much to extinguish.

Since they had concluded that the reinforcing effect of light onset was not due to the duration, intensity, or change in intensity, but was due to a change in stimulation, Morris, Crowder and Crowder (1961) conducted an additional experiment of essentially the same design as reported above, except that they varied the position of the light onset rather than the intensity. For one group, the reinforcing light was on the left side of the lever, for another, on the right side of the lever. and for the remaining group the light was alternated from left to right on successive trials. Once again performing their analysis on the number of responses during an extinction period, they found an insignificant tendency for the rats that had the light in a constant position to emit more responses.

Barnes, Kish and Wood (1959) contrary to the findings of Morris _et al_., (1961) found that intensity of light was an important variable in the number of responses it would sustain during a conditioning trial. Using inbred male mice, they also arranged light at different intensities to be contingent upon a lever response and found a concave, downward curve when they plotted the number of responses against the intensity of the light.

Steward (1960) very carefully conducted an experiment to explore the relationship between intensity of light and schedule of reinforcement as determinants of response rate. She used hooded male rats in experimental boxes that were totally darkened for the thirty-minute experimental sessions. After a period of two weeks of habituation to a hunger regime that permitted free access to food for one four hour period daily, she conducted five sessions to determine the operant rate of the lever response and found a significant decline during the five days. Following this, she introduced four intensities of light as reinforcing stimuli for four groups of eight animals. This continued for 17 sessions of continuous reinforcement. Under continuous reinforcement, the groups experiencing different light intensities were not differentiated by the number of responses made during the experimental sessions, although all groups performed at a significantly higher rate than the basic operant level.

Following the continuous reinforcement trials, Steward (1960) changed the animals to 2:1 for two sessions, and then to 3:1, 4:1, 5:1 and 6:1 schedules of fixed ratio reinforcement for successive experimental sessions. These conditions resulted in a significant trend (p. = 0.02) for reinforcement ratio and intensity to interact to produce a higher rate of response. During extinction, the rats performed at a higher rate than they had during the last operant trial, and the number of responses was generally related to the rate at which the rats had performed during the experimental sessions. The latter had been a function of both light intensity and ratio of reinforcement.

The early studies of light onset as a reinforcer were both stimulating to research and tantalizing in their implications, but some research workers have suggested that the phenomenon may not be so clear-cut as it first appeared. Crowder, Walker and Crowder (1960) suggested that photo-reinforcement might be a facilitation phenomenon rather than a learning phenomenon and undertook an experiment to show that this was so. What they did was to undertake a typical bar pressing experiment with rats using a weak light as a reinforcer and then a day later tested for the retention of the habit.

They argued that if the increase in bar pressing rate during photo-reinforcement was only a facilitation phenomenon then it would not show up in the retention test. This is exactly what happened. A weak light, the occurrence of which was contingent upon bar pressing, resulted in an increase in the rate of bar pressing, but no evidence could be found for the retention of this skill. Crowder, Walker and Crowder suggest that this is evidence consistent with the hypothesis that the contingency of light facilitates rather than reinforces. In an additional similar experiment Nash and Crowder (1960) arrived at a similar conclusion, but were willing to concede, despite their experimental results, that weak light may function as a reinforcer under some conditions, but that the nature of these conditions had not yet been determined.

The argument that lack of retention shows that photo-reinforcement is an artifact may not be entirely sound. At best, the increase in response frequency with light-on or light-off as a potential reinforcement is quite small and the expectation that a part of this small increment will be retained by a rat over a period of 24 hours may be unrealistic. In some studies, where intensive daily training with light reinforcement has been provided over a period of ten or more days, as in that by Steward (1960), rats have shown a retention of the operant level acquired during training. The rats have also shown genuine extinction phenomenon. While there may be a facilitation effect operating, genuine learning through reinforcement also appears to occur.

If photo-reinforcement is accepted as a genuine phenomenon, then this leads to controversy concerning its nature. The most immediate suggestion is that it involves a secondary reinforcer deriving its properties from having been paired with the incident of food or with some other primary reinforcer. Direct experimentation concerning this hypothesis can be undertaken and a study by Roberts, Marx and Collier (1958) provides at least a tentative answer. The latter research workers report that rats raised in darkness from the time of weaning do not show any less of a tendency to be reinforced by low level illumination than do rats raised under typical levels of illumination. This would not be so if illumination functioned only as a secondary reinforcer. However, these results do not mean that light may not function as a secondary reinforcer on some occasions. Henderson (1957) has clearly shown that by pairing light with the availability of food, the light acquires some secondary reinforcing properties. Nevertheless, Henderson could not account for all of the reinforcing properties of light in terms of previous occurrence in the presence of a primary reinforcer. Another experiment by Hurwitz and Appel (1959) also throws some light on this issue. Hurwitz and Appel (1959) placed their animals on a 12 hour light-dark cycle, and fed one group during the last two hours of darkness and the other during the last two hours of light. After two weeks on this schedule, they placed the rats into the experimental boxes for daily half-hour operant sessions. The two groups were not distinguishable on the basis of their operant rates of response. Following the operant sessions, a bar press resulted in light onset of a six-volt light covered with red plastic. During the first reinforced session, the rats fed during the light part of the light-dark cycle made significantly more responses than the dark-fed rats, but there was no appreciable difference between the groups during the second session. These results suggest that there may be a temporary secondary reinforcing effect due to the past association of light with food.

The weight of the evidence is that weak light functions as a positive reinforcing contingency with the species studied and that it is not necessarily a secondary reinforcing phenomenon resulting from the pairing of light with food though such pairing may enhance the effect. The phenomenon is also not just a facilitation phenomenon which emulates reinforcement and learning phenomena.

The fact that weak light functions as a reinforcing stimulus does not fit well with any widely endorsed contemporary theory of learning. This has led to speculation concerning the nature of such reinforcing phenomena, both their genesis and modus operandum. Hurwitz (1960c) raises, discusses and rejects the possibility that light stimulation of the type provided in the experiments reported is simply one form of novel stimuli, all of which have reinforcing properties. He points out that the novel stimulus theory collapses in this application since it implies that the novel stimulus, in this instance, light, would lose its novelty with repeated use and thus lose its reinforcing properties. But this does not happen; light onset continues to have reinforcing properties even after continued use. Another position that has had several proponents is that it is not so much the novelty of the stimulus that accounts for the reinforcing properties as it is the simple fact of stimulus change. This position, most clearly expressed by Kish (1955) and Marx, _et al_., (1955), would account for the continued efficacy of light as a reinforcer, but does not account for the fact that light onset is a better reinforcer than light offset (Roberts et al., 1956; Robinson, 1959). On the other hand, it is not necessary to account for the phenomena with any one theory. Many studies have shown a decrement in the reinforcing efficacy of the stimulus even though the effect is not lost. This could be attributed to a loss of the novelty value of the stimulus. Robinson (1961) points out in his article that two assumptions are required to account for the rest of the findings. The first, the Marx-Kish hypothesis, is that stimulus change provides reinforcement. In addition to this he states it is necessary to posit that an opportunity to scan the environment is an important determinant of this type of behavior. This, of course, is fully consistent with the data afforded by the studies on exploratory behavior.

An attempt has been made by Bevan and Adamson (1960) to formulate a theory involving stimulus change as the basic event in reinforcement phenomena. However, according to these authors, it is not stimulus change, as such, which has reinforcing properties, but change in relation to an internal norm, or level of adaptation to use Helson's term. Thus, a weak light will be a poor reinforcer for a light adapted animal, but a relatively powerful reinforcer for a dark adapted animal. Bevan and Adamson have collected some evidence to demonstrate that at least under some conditions the relationship expected between variables in terms of the theory actually occur.

Summary

Alternation behavior has been a phenomenon of particular interest to psychologists, partly because it does not fit easily into the reinforcement paradigm and partly because it appears to provide an instance in which behavior is controlled by environmental characteristics. Drive-reduction reinforcement theorists have generally accounted for alternation behavior in terms of the principle of reactive inhibition, but the bulk of the evidence indicates that the phenomenon is not one of response inhibition and response alternation. Data support the position that alternation behavior involves place and position alternation, rather than response alternation.

Alternation behavior can be regarded as a special case of the larger category of behavior known as exploratory behavior. The main difference between the two lies in the fact that the former occurs in two-choice situations where the choices are a matter of place or position, while the latter occurs in situations in which there are many different responses open to the organism and the alternatives differ in stimulus characteristics in addition to that of position.

The early studies of exploratory behavior tended to emphasize its internal origins with the suggestion being commonly made that organisms manifested an exploratory or curiosity drive. Later studies placed their emphasis on stimulus condition and involved the use of the same T-maze and Y-maze that had been used in the study of alternation behavior, but the stimulus characteristics of the arms beyond the choice point were varied. From such studies two explanatory concepts evolved; one was that approach behavior occurred to novel stimuli, and the other was that animals avoided familiar stimuli or became satiated with them. Some evidence has been accumulated that both such factors are operating and that they are dependent upon learning.

The exploratory behavior of rats has been demonstrated experimentally to be related to the complexity of the environment, rats move into the more complex parts of their environment. Fear responses are related in a complex way to exploratory behavior. It is well known that fear may produce both approach and withdrawal responses to novel stimuli. Both of these responses to fear producing objects may have survival value, but the conflict involved may also virtually immobilize the animal. Conditions which reduce fear in experimental situations may increase the amount of exploratory behavior. The study of the fear variable is complicated by the fact that there are differences among strains of rats in the amount of exploratory behavior manifested suggesting that a genetic variable may also play an important role.

It seems well established that the general level of activity and, hence, the amount of exploratory behavior depends upon internal as well as external conditions. However, the relationship between exploratory behavior and need states is not well established.

That exploratory behavior or the related stimuli can be used to reinforce behavior seems now to be well established. Many experiments show that animals seek out environments which include novel objects and learn to locate those parts of the environment which contain such objects. The simple experimental procedures used for demonstrating this are like those involving Richter tubes in which it can be shown that animals prefer to consume certain substances rather than others. However, exploratory behavior has been shown to have reinforcing properties in more complex situations and has been used to reinforce discrimination learning. In the studies of exploratory behavior animals are shown to _prefer_ those parts of their environment which provide novel stimuli. This phenomon fits well Premack's (1959) conception of reinforcement if one interprets behavior having a high response rate as a reinforcer. One can equally well interpret the situation as one in which the stimuli related to the exploratory behavior are the producers of reinforcement.

A related phenomenon to those that have been discussed is photoreinforcement. It is related in that it is an example of a case of stimulus control of behavior. Even under favorable laboratory conditions it is a weak type of reinforcement and, for this reason, some studies which have had difficulty in producing the phenomenon have sometimes concluded that it is some kind of experimental artifact, or that it is a phenomenon of facilitation rather than one of reinforcement. The weight of the evidence shows that it is a genuine phenomenon and that it is produced by change in illumination level. It is of particular interest in the development of reinforcement theory in that it represents a reinforcer which is neither related to the reduction of what are ordinarily listed as primary needs and neither can it be considered as a secondary reinforcer. It fits reinforcement theory in so far as the latter defines reinforcement in terms of stimulus events, but does not fit the drive-reductionist conception of reinforcement.

Positive Reinforcing Stimuli: Social Stimuli

The word social is arbitrarily restricted here in meaning to apply only to those situations involving two or more members of the same species, two rats, two dogs, two people, etc. It is somewhat arbitrary, of course, for it does exclude from consideration the strong bond of affection that might develop between an experimenter and the animal he runs through the maze, a man and his dog, or a child and his pet. But at the same time, the quality that sets their relationships apart is retained as an integral aspect of a social situation. Specifically, social will be restricted to apply only to those situations which are characterized by the fact that, because of past experience with other members of the species, at least one of the members has certain expectations or attitudes concerning the other. Thus those situations involving man and wife, mother and child, or a group of children are social situations. So too, although it is often neglected, is the situation of experimenter and subject, if, as is so often the case, the subject happens to be human. A social situation, in short, is one that maximizes the importance of the history of the organism. Without some sort of history with other members of the species, another member is simply a curiosity, a source of novel stimulation.[1]

But, other members of a species have been a source of nurturance, comfort, approval, praise, punishment, pain, and frustration. And surely the complexity of the matter is compounded by the intervention of language. The works of both Osgood (1956) and Bruner (1957) suffice to demonstrate that the subjects' perception of an experimental situation is greatly influenced by prior learning, and that the individuals response is largely a function of his particular perception. Consider, for instance, the following poem collected by Smith (1940) during the course of his excellent experimental analysis of poetry writing:

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[1] It should be acknowledged that this does some violence to the splendid work that has been done in comparative psychology. It has been demonstrated that certain stimulus configurations that are specific to a species have the ability to elicit, without specific training, rather specific responses.

The Psychology Test

My fingers are stiff, and the library's cold,
The ink's clogged, the blotchie's old,
My brain's empty, the time's short,
And I'm to write a poem of a sort.

Pink paper, and mocking phrases,
One of these Psychological crazes...
Scrawny men who try to find
What goes on inside the mind;

I could tell them well enough,
But they'd rather mess with stuff
Not concerning them or us.
Oh! For heavens! why this fuss.

They can have their "dewy roses"
And any amount of Victorian Poses.
But from me, instead of verse,
They receive a hearty curse.

Who can deny that the poem reflects social determinants, that the structure of the response, so to speak, is a function of the structure of the social situation? The subject's percepts of the role, motives, and methods of the psychologist are amply evident. And the poem is all the more interesting since the test was administered by school personnel. It is unlikely that the subject ever met the psychologist.

It is very easy to recognize situations, as in the above illustration, that have maximized the history of the organism, and for this reason social stimuli are frequently mentioned and have occasionally been studied on an experimental basis. But there has as yet been no extensive, systematic investigation of the dimensions of social situations that are of particular importance in learning, in spite of the fact that typical educational programs are conducted almost exclusively in this type of setting.

Social Facilitation, Imitation, and Vicarious Learning in Subhuman Organisms

One of the simplest social conditions is accomplished by including a second animal in a standard experimental situation. Consider, for example, an experimental arrangement which necessitates the performance of an instrumental act like those performed by Small (1900). He would introduce an experimental box, a problem box, into a home cage. A typical problem box would contain food, but would be completely enclosed except for one small entrance that was obscured by sawdust which banked all the sides of the box. The animal, then had to learn the location of the entrance and dig through in order to obtain the food, the value of which was ensured by appropriate conditions of deprivation. This sort of task is probably too complex for an exhaustive analysis of behavior, but Small would record the time it took the animal to get the food on successive trials. Time scores did, indeed, show improvement with practice. Now when he introduced a second rat into the situation, such that there were two rats at a time in the situation, he did *not* find that two heads were better than one. As a matter of fact it took two rats about the same length of time to solve the problem as it did for one rat. It was only when he finally used a pair of rats that were fairly readily distinguishable that he noticed that one rat consistently solved the problem while the other learned to take the food from the first rat. The first rat would then return to the box for another piece of food. He subsequently marked his rats and found this to be a consistent phenomenon; the problems posed for the two rats differed markedly. Even with two rats in the box, only one learned to solve the puzzle-box problem, and his performance did not improve by virtue of the fact that he had a friend that would take his food.

The implications of the study are very important in a general way. It should be noted that the animals were food deprived, thus the stimulus or incentive value of food was maximized relative to the other stimulus elements of the environment. With only one subject in the experiment, the problem of getting the food would necessarily entail the solution of the problem the experimenter had set for the subject. But with the introduction of an additional subject, the solution of the problem by one (which resulted in food being available in two places in the environment) afforded an alternative solution (to the problem of obtaining food) for the second animal. The relationship between these factors should be stressed. The task is determined by the particular condition of deprivation, the type of incentive used in the experiment, and the means available for attaining the incentive or reinforcing stimulus.

In addition to modifying the task as in the experiment just cited, the introduction of additional subjects into an experimental arrangement provides another source of stimulation, stimulation that may elicit responses that are incompatible with the experimental task. This is roughly what happens when the children in a class are attending to the classroom comic. A similar effect was demonstrated by Voget, Scott, and Marsten (1950). Using a straight alley maze, they provided reinforcement in the way of food or petting at the end for 16 dogs. Compared to the speed of the animals when run with a familiar partner, the introduction of an unfamiliar partner resulted in 73 percent interference.

There has been considerable interest aroused in other dimensions of social situations. One of these is the problem of social facilitation of responses. One of the early investigators in this area, Harlow (1932), had concluded that response facilitation was a function of a competitive situation. In his own work, he had observed that puppies that ate separately would eat less than those that ate together. Crawford (1939) noted the social character of this and related experimental situations and defined social facilitation as an increment in activity that can be attributed to the presence of another animal. But earlier studies resulted in contradictory results.

Lepley (1937) and Bruce (1937), for instance, found that the presence of a second rat facilitated a running response (after an initial decrement in the case of the data produced by Bruce), but Lepley (1939) later failed to find evidence of social facilitation. Water (1937), when he trained rats singly and in groups on enclosed and elevated mazes, also found no evidence of social facilitation. Mowrer (1941) and Winslow (1940) demonstrated that the mere presence of another animal will not necessarily facilitate a response, but that the effect of the presence of another animal often depends to a large extent upon the history of the animal. They both used a training situation in which the animals were in competition for food that was offered. In this sort of arrangement, consistent winners would run faster when another rat was present, while losers would run more slowly or stop. The importance of the learning process in the behavioral consequences of social conditions was further investigated by Holder (1958). She used an apparatus that provided two detention boxes on one side of a square and a food trough on the other side. Three different experimental conditions prevailed during the training. For one group (frustration), the experimental and social subjects were released simultaneously on trials that were rewarded. The complementary arrangement, a social animal released on the rewarded trials, but not on the unrewarded trials formed the reward group. A third group had the company of a social animal on half of the trials, while rewards were distributed randomly. As the experimenter suspected, the social animals functioned as a discriminative stimulus. The experimental animals from the reward group ran faster on the trials during which the social animal was released, while the frustration group ran more slowly on these trials. The control group showed no difference in response speed that was attributable to the presence of the social animal. In a later phase of the experiment, the experimental animals were able to release the social animals from a detention cage by pressing a lever. As expected, the latencies for a lever press were far shorter for the rewarded group than for the control group. The frustration group was least likely to press the lever. It would appear, on the basis of these results, that competition is not necessary for a facilitory effect, but that the effect can be attributnd to the previous learnings of the experimental animals.

Response facilitation bears a strong relationship to another type of social phenomenon, imitation. Small (1900) found that the variability of the behavior of several rats in his experimental session was far less than would be expected to occur. Rather, if one rat started to dig, the remaining rats would dig also. Any shift in behavior by one rat was quickly followed by a shift in behavior by the remaining rats. On the other hand, there was no evidence of imitation of behavior in terms of establishing means-end relationships. When a rat that had been successfully solving the problem of the type mentioned above was eliminated from the group, it did not appear that the remaining rats had profited greatly from the opportunity to watch the lead animal successfully solve the problem during many previous trials.

On the other hand, imitative behavior can be established on an experimental basis. For instance, Miller and Dollard (1941) reported an experiment that called for imitative behavior. They trained food-deprivated rats to turn left or right on a simple elevated T maze at the discrimination of a black or white card. Beginning on the stem, the rats ran to the choice point where, if correct, they responded to the black card at the end of one of the arms. Food was to be found in front of the black card while no reward was paired with the white card. The next procedure was to place naive rats directly behind the color-discriminating leaders. These were not found to have an initial tendency to imitate, but such a tendency developed gradually as the result of being rewarded for this behavior. After twelve days of training, rats showed an ability to imitate a leader at a level of statistical significance ($p<.001$).

Another group of rats was rewarded for non-imitation by placing food in the arm not chosen by the leader rat. This group learned comparably well (at a high level of significance) to not imitate, that is, to make a choice opposite to the one of the leader. A further finding of considerable interest is that the rats tended to generalize the tendency to follow the leader, often following rats of quite different appearance.

Another experiment that has many characteristics in common is one reported by Darby and Riopelle (1959). They used two Rhesus monkeys, one of which alternately served as an observer (O) while the other served as a demonstrator (D). Employing a problem-box with a restraining observational compartment at the side, they first allowed the demonstration monkey to remove a puzzle object covering one of the two holes (food wells) and then gave the observing monkey access to the puzzle.

During the demonstration, food had either been in both wells or in neither. The question asked of the experiment was whether or not the (O) monkey could respond appropriately following the (D) monkey's experience, viz., if no food was in well "A" for(D's)trial, would (O) go to "B" food well when given his turn? The only occasion upon which the observer was rewarded occurred when he made the same response as (D), if (D) had received reward, or, if (D) was not rewarded, when the (O) monkey had made the opposite choice for food. In all, 500 discrimination problems were set up for each pair.

All monkeys, having alternately served as observers, showed improvement in ability to acquire information by observation. Performance on the first test trial was 52 percent or chance. At the conclusion of the experiment it was 75 percent and still rising. An interesting finding was that observational-learning was greater if the demonstrator monkey had made an "error" than if he had responded "correctly."

It can be noted in the two studies that control over the behavior of the animals was accomplished much in the same manner as in other discrimination learning problems. The behavior of the other animal provided a cue for the correct response which was subsequently reinforced with food. There is nothing dramatically different in these experiments. It is also interesting to note that imitative behavior seems in no way to have an edge on other types of behavior, for it was just as easy for the animals to learn to not-imitate as it was for them to learn to imitate.

There are other conditions that conduce to imitative behavior, but the conditions of the experiments have typically been sufficiently complex that it has been impossible to explicate the underlying mechanisms. This is true of the social conditioning study reported by Angermeier et al. (1959). In their procedure, two animals were placed in a starting box. At the sound of a buzzer, the door of the box was opened and the animals were simultaneously shocked by means of a grid in the floor. The animals were permitted to escape through an alley to another box. When escape behavior had been reliably conditioned to the buzzer without shock, a third rat was placed in the box with the other two. Ten trials per day without shock were run with the three rats. This was done until the naive rat would, at the sound of the buzzer, run five of the ten trials with the rats conditioned to shock. The naive rat was then tested alone to determine if the escape response would occur as many as five times without the model as stimulus.

Three rats were used as controls. These were never shocked, but were tested for an escape response to the bell. Analysis of variance comparing shock-naive, but socially conditioned, rats with the controls indicated a highly significant difference ($P<.001$) in the number of runs at the sound of the buzzer when the rats were run individually. Although the rats did, in a very real sense, imitate the behavior of the leaders, it is difficult to interpret these results in terms of discriminative learning, for there was no physical reward offered for imitative behavior. It is also difficult to interpret the results within the framework of a social-conditioning paradigm that employs the concept of another animal serving as a positive reinforcer. Most likely, the results are to be accounted for in terms of avoidance learning. There is some evidence that animals commonly perceive danger signals in the behavior of other animals. This, in itself, could induce a condition of anxiety that would be subsequently reduced as the rats departed from the scene with the conditioned animals. The subsequent reduction in anxiety (as the conditioned animals became less fearful) would serve to reinforce the response. Within this model, then, imitative behavior would still be a function of the history of the animal.

The fact that the studies just cited can be interpreted in terms of the learning of a discriminative stimulus is ample evidence that the entire issue of imitative learning has been deftly skirted rather than resolved. Thorndike (1898), in his pioneering studies on imitative behavior, was directly attacking the prevalent notion that there was an instinct of imitation that accounted for cultural transmission. The fact that imitative behavior was not observed in rats by Small, nor in chicks, cats or dogs by Thorndike in no way precludes the possibility that it could function as a powerful mechanism among species higher on the phylogenetic scale, or even with lower animals if greater control were exercised over the conditions of observation. The important issue of this point seems mainly to be whether or not it is possible for one animal to vicariously profit by the experience of another. Stated somewhat differently, will an opportunity to observe another animal learn to solve a problem facilitate the solution of the problem by the observing animal? It was this question that was attacked by Herbert and Harsh (1944) who, incidentally, provide an excellent review of the topic. They presented five problems to cats, some of which were learning to solve the problem for the first time and others which had had the opportunity to observe the initial efforts of the first cat that solved the problem. Some of the observing animals observed both the learning trials and proficient trials, while others observed only proficient performance. In every instance the cats that had done the most extensive observing were far more proficient in solving the problem than were the cats that had witnessed only proficient performance and the cats that learned the solution without prior observational experience. The observation of proficient or skilled performance only was an effective means of improving performance on only two of the five tasks.

Imitation and Vicarious Learning in Human Subjects

The results obtained by Rosenblith (1959) are of particular interest both in terms of generality and in the matter of demonstrating the complexity of the problem of imitative and vicarious experience. In addition to her interest in imitative behavior, she was concerned with the interaction of sex differences between the demonstrator and subject, and in the importance of giving the subject attention prior to the trials. She used kindergarten children and arranged for an adult (the demonstrator) to play a "game" with each child subject. The experimenter called both the model and child subject by first names and instructed them to work together. The adult model initiated the learning by going through the task (Porteus Maze test) first.

As the child watched, the adult proceeded, but with some hesitation at choice points.

The subject was then tested for his own performance.

The results showed that:

1. Having a model was more effective than additional trials of the same number as in the demonstration.

2. The male leader was more effective.

3. Boys showed more improvement over initial trials than girls.

4. Attention to the child given by the model prior to the demonstration had a positive effect on learning provided that S and model were of the opposite sex. When of the same sex, boys, but not girls, do better if attention is withdrawn for half of the time.

Further evidence of learning through vicarious experience is given by Lewis and Duncan (1958). These researchers have used a "one-arm bandit" gambling device in a number of studies of the partial reinforcement effect (PRE). This effect is defined by persistence in a given response during extinction trials after partial reinforcement. The effect is obtained by reinforcing an experimental group occasionally, i.e., 25 percent of the time, and a control group 100 percent of the time. When reinforcement is subsequently withheld from both groups, the 25 percent group may be expected to perform the unrewarded act a larger number of times.

In the study relevant here, the authors wanted to see if the partial reinforcement effect would maintain for people who simply watched "one-arm bandit" lever pulling, or in another case heard a hypothetical description of an individual playing a "one-arm bandit," when the description was given in commentator fashion as though the events were actually occuring.

The data showed the typical partial reinforcement effect for control subjects who actually played the machine. A measure of perseveration was also obtained from eight vicarious groups by allowing them to actually play an extinction series themselves. The results of the four analyses of variance showed that:

1. The partial reinforcement effect was as great for subjects who watched the control subject and won when he won as it was for the controls who actually played.

2. The partial reinforcement effect occurred also for subjects who heard a hypothetical description of playing, ($p<.01$), but only if they won when the hypothetical player won.

3. Subjects who watched only, or heard a description only, did not exhibit the typical partial reinforcement effect as a result of intermittent reinforcement.

Evident in this study is a less potent learning outcome as a result of vicarious experience than with learning by direct performance. This is seen by the fact that the partial reinforcement effect resulted only when vicarious Ss' received a winning disk at the same time as did the active player. Such disks could later be exchanged for money. In this case the act of receiving reward was not vicarious but actual. How much of the learning in groups that won with the control subject was due to vicarious experience is still to be answered.

In 1958 Kounin and Gump investigated the vicarious impact of discipline. When discipline applied to one pupil had an effect on another, these authors called it the "ripple effect." Subjects used were newly matriculated kindergarten children. What the investigators wanted to determine was the consequence of reprimand upon children in close proximity to the one being admonished. What attitudinal changes occur in B when A is disciplined? Measurement was in terms of observers' rating. Fifty-one undergraduate students were "carefully instructed on their assignment" to observe the effect of teachers' chastisement for misbehavior. Three dimensions of assessment were used to evaluate reprimands and the consequent behavior of children in close proximity. These were clarity, firmness, and roughness.

Results of a chi-square test of observers' data showed that clear statements of what the child was expected to do had the effect of winning the conformity of children nearby. Unclear or ambiguous reprimands had the effect of increasing non-conformity. The test was significant at the .01 level.

Firmness, as rated by observers, was not a significant factor in the behavior of children who stood nearby and watched. By knowing the firmness of a teacher's technique, it was not possible to predict the extent to which the reproach would transfer to adjacent children.

Roughness on the other hand, resulted not in greater conformity and less non-conformity, but in disruption. Though roughness was described as upsetting, the authors do not describe what was actually meant by "disruption" or by "upsetting."

Children who were misbehaving themselves when a reprimand was given to another, were more responsive to vicarious control than those indulging in appropriate behaviors.

Social Conditions as Reinforcers of Animal Behavior

The various dimensions of social conditions that have relevance for studies or reinforcement have been briefly surveyed, but to this point the use of social stimuli for the purpose of reinforcing a response have been neglected. It was mentioned at the outset that a social situation was one that maximized the importance of the history of the experimental animal. This was demonstrated by Angermeier (1960) in a study that showed a differential reinforcing effect accruing from a social history. Twelve male rats were reared in isolation from one another from the time of weaning to 120 days when the experiment began. A second group of 12 male rats were reared together. During the experimental session each rat was placed in a modified Skinner box in which the pressing of a bar resulted in the raising of a door permitting either visual, partial physical contact, or full physical contact with another rat. In half the cases the rat thus seen or contacted was male and in the other half female. The time of exposure was 10 seconds. Acquisition sessions were followed by extinction sessions. The results of this study showed that all the animals reared together learned the response. In the group reared apart learning increased directly with the degree of physical contact. Angermeier concluded that the involvement of tactile receptors in a social situation, both during rearing and learning, was largely responsible for the effects observed.

Of particular interest in the interpretation of the Angermeier studies is the fact that rats which are generally considered to have only the most limited social needs should manifest learning by social reinforcement.

Working with a much more social species, namely, Rhesus monkeys, Butler (1954) was able to show that the mere sight of another monkey could function as a strong reinforcer. Butler used a box from which a monkey could view the room outside by opening a small window. Through the window the monkey might see either an empty room, or food, or a toy train in action, or another monkey. Butler showed that the monkey opened the window more frequently when the other monkey could be viewed than under any other conditions. The experiment is similar to those involving Richter tubes in that the data reflect the animal's preference for particular stimuli. One can perhaps reasonably assume that the monkey's preference for stimuli is related to the reinforcement value of these stimuli in other situations and on other occasions.

Social Reinforcement of Simple Motor Skills in Human Subjects

A great range of studies are included within the category of social reinforcement in human subjects. These include phenomena which range from the reinforcement of very simple motor acts to the reinforcement of complex behavior manifesting attitudes as well as the shaping of verbal behavior itself. The social reinforcement generally provided is a comment of approval which may be an outright statement such as "That is very good" or a mere grunt or nod of the head. Such reinforcers are very difficult to control since the same comment may have different reinforcing values depending upon the tone of voice or the manner in which they are administered.

Stevenson and Cruse (1961) studied the effect of social reinforcement on the maintenance of behavior involved in a simple motor task which involved the placing of marbles of one color in one hole and marbles of another color in a different hole. This is a task which rapidly extinguishes in children of school age of normal ability unless reinforcements are provided. Stevenson and Cruse administered the task under three conditions: (1) with supportive comments after every fifth response, (2) while E watched S, but with no comments, and (3) when S was alone in the room. Subjects were either normal children of kindergarten age or feebleminded children averaging 14.2 years, with the same mental age range as the normal children. The normal children responded differentially to the reinforcing conditions and showed the greatest persistence with the task under the sound approval condition. The feebleminded children showed no differential response to the three situations, but displayed an extraordinary persistence with the task under all three conditions. However, in a second experiment in which a fourth experimental condition was added, namely punishment, such clear-cut results were not found.

The results of the first Stevenson and Cruse experiment add comparison rather than clarity to the picture, for Zigler et al. (1958) had previously found almost the reverse effect; feebleminded children responded to social reinforcement while the normals failed to do so. Zigler et al. also used simple tasks, but they were different from the one used by Stevenson and Cruse, so it is possible that the discrepancies can be accounted for in terms of task differences. In support of this is the fact that Stevenson and Cruse noted that the feebleminded children tended to show a high degree of persistence with their task, even in the absence of any reinforcement manipulated by the experimenter. Responses to such a task would undoubtedly be relatively uninfluenced by reinforcements extrinsic to the task.

The Zigler et al. (1958) study confounded institutionalization and feeblemindedness, conditions which a later study by Stevenson and Fahd (1961) managed to keep seperate. The latter also introduced a factor of task complexity into their design. The simple task involved in insertion of a single marble into a single hole, while the complex task required the subject to insert a marble into any one of six holes. Here again two reinforcing conditions were provided; in the one case the experimenter was attentive, but unresponsive, while in the other he made comments of approval. Although the experiment involved a large number of subjects (N=124) no significant differences were found between normal and feebleminded subjects; on the other hand, whether the children were or were not institutionalized did make a difference. The reinforcement condition (attentive versus approving) was not significant as a main effect, but did enter into a significant interaction involving task complexity and institutionalization. The importance of task variables may well be the factor which accounts for inconsistent results among different experimental researchers.

Gewirtz and Baer (1958a, 1958b) have attempted to study social reinforcements with children within a framework of drive reduction theory. In an initial study (1958a) these research workers demonstrated that social approval could reinforce a response of placing a marble in one hole rather than in another. In a second study (1958b) they attempted to manipulate social need as a "drive" factor by exposing three groups of children to three different social conditions prior to exposure to the marble task. These conditions involve isolation (deprivation), a set of "normal" social conditions, and excessive social approval (satiation). These three conditions produce the expected effects on responsiveness to social reinforcement. Another approach to the study of the effects of social deprivation on responsiveness to social reinforcement is provided by Zigler (1958). In this study Zigler compared the effects of social reinforcements in institutionalized children divided into a group that had been highly deprived prior to institutionalization and a group that had been less deprived. Differences were not significant at the .05 level of confidence.

Comments of approval, contingent upon the manipulation of particular objects, have been shown to change the preference value for the objects. For example, Hunt (1955) asked children to rank toys in order of preference. Later each child was asked to perform a task with one of the toys under one of two conditions. One condition involved comments of approval for the child's manipulation of the toy, and the other involved no comment. Later, the toys were reranked and the condition of social approval was found to influence the ranking of the toys involved.

Studies have been undertaken to determine whether social reinforcers are more effective at one age level than at another. In order to conduct this type of study the research worker must find a task which can be undertaken at the different age levels involved and which is sensitive to the effects of common reinforcers. Several tasks were designed for this purpose. One such task involved the pushing of knobs, another required that marbles be inserted into holes.

McCullers and Stevenson (1960) using a knob pressing device compared a 3-5 year old group with a 10-year old group. Half the group were reinforced with a marble (on a partial reinforcement schedule) for pressing a particular knob, while the other half were reinforced with a marble plus a reinforcing comment from the experimenter. The younger group was found to be influenced more by approval reinforcements than was the older group.

Stevenson and Cruse (1961) made a comparison of five and 12-year old children on a task involving the insertion of marbles in a hole and introducing social reinforcers (approval comments) with some of the subjects. Here again the five-year old group was more responsive to these social reinforcements than was the older group.

Social factors may be introduced when non-social reinforcers are applied. For example, a pupil may be informed that his response is right or wrong either by being given a light signal mechanically or he may be given the same signal through the action of a human operator. Whether these two conditions differ in reinforcement value is an important issue which has high relevance to the use of mechanical teaching devices. A single study by Sidowski (1957) provides a beginning to the answering of this question. Sidowski used a button pressing task which was administered under three conditions: (1) with occasional shocks, (2) with a score indicating how well they were doing, and (3) with both shock and score. The subjects were divided into two groups, one of which was instructed that the shock and scoring procedure was under the control of another person in a different room, and the other group was led to understand that these were controlled mechanically. The social condition, thus introduced, did not show any evidence of influencing learning over and above that provided by the other conditions of reinforcement, but there is presently no reason for believing that the other conditions were not themselves social reinforcers. Verplanck (1956), in his very timely article on operant conditioning of human motor behavior, noted that any discriminable stimulus, with appropriate instruction, can serve as a reinforcing stimulus, but that the reinforcing efficacy of the stimulus is dependent upon the general reinforcing value of the experimenter. In his early studies, he found that students varied considerably in their ability to produce conditioning; the success of the students seemed strongly related to their personal characteristics; the more socially acceptable (best looking, more socially mature) students producing the greatest effect. But, given a condition in which the student was motivated to perform for the experimenter, any signal, for instance the tapping of a pencil, would suffice as a reinforcement of the response being conditioned. He also found, incidentally, that college students would work for 'points' in a situation in which they received one point each time the experimenter tapped the pencil. He found much more uniformity in results when the students were working for points than he did when they were responding to the more strictly social consequences of the response of the experimenter, the importance of the differences in the social characteristics of the experimenters being minimized under these circumstances.

In another study, Verplank (1955) provided data that are very important because of the generality they lend to other findings. It has probably been noted that most of the studies reported in this area have been dealing with fairly specific and well defined responses that, generally, are of no practical consequences. It is particularly interesting, then, to find that he has been successful in conditioning a general response class, the expression of opinions that permitted variability in particular responses. The positive results he achieved confirmed the assumptions that the expression of opinions is a class of behavior that acts like a response (e.g., is conditionable) and that agreement or paraphrasing acts as a reinforcer of the response class. On the other hand, if reinforcement was withheld, if there was no agreement or paraphrasing, the rate of expression of opinion declined. There are undoubtedly other classes of behavior, thus lending to the generality of the reinforcing effect, but experimenters have not yet given concerted effort to this problem. Indeed, the question of what it is that essentially binds discrete responses into a general class of behavior, although of considerable importance, remains essentially unexplored.

Social Reinforcement of Attitudes

The studies considered up to this point have involved mainly the social reinforcement of motor skills. They demonstrate that a wide range of living creatures may have their behavior modified through visual, auditory, or tactile relationships with another member of their own species, and that this reinforcement phenomenon is dependent upon previous social experience. The studies covered up to this point have dealt with the reinforcement of various forms of motor skills, and have not yet touched upon the most extensively studied area of social reinforcement, namely, social and verbal behavior.

In the early thirties Edward L. Thorndike had begun to speculate that interests and attitudes were learned by the same laws that apply to the learning of verbal habits and intellectual and other skills. The law of effect was considered to represent the central mechanism operating, and experiments were undertaken by Thorndike and his associates (1935) which demonstrated that certain aspects of these phenomena, namely, aesthetic preferences, could be modified by appropriate conditions of reward. This work did not attract the attention it merited, and not until the years following the World War II was any extensive interest displayed to this approach to the study of attitudes.

Reinforcement-learning theorists would predict that the most effective way to change attitudes would be to arrange in some way for behavior related to the new attitude to be elicited and then to reinforce that behavior. Of course, the difficulty generally encountered in following such a procedure is the practical one that usually there is no method available of eliciting the desired behavior. Nevertheless, the matter is one of great theoretical interest, and psychologists have attempted to design experiments to test this theory of attitude change.

Scott (1957) designed a study to determine whether a situation in which a person was required to make a statement with which he did not agree, but which was reinforced, resulted in a change of attitude in the direction of the reinforced statement. This situation was an ingenious one. Students were required to engage in debates on various issues and were assigned the tasks of presenting speeches representing points of view with which they did not agree. In some cases the speech was followed by a reinforcement, which consisted of a purported vote indicating that they had won the debate. Post-tests of the attitudes of the students indicated that those who were named winners of the debates showed a significant change of opinion toward the position they had presented in the debate, but the so-called losers had not changed significantly.

The interpretation of these results in terms of reinforcement theory would be that the presentations at the debates were reinforced and that this generalized to related expression of opinion.

Festinger and Carlsmith (1959) conducted another study which demonstrated a similar phenomenon. In this study the hypothesis was that "if a person is induced to do or say something which is contrary to his private opinion, there will be a tendency for him to change his opinion so as to bring it into correspondence with what he has said or done. The greater the pressure used to elicit the overt behavior, the weaker will be the tendency." In this study subjects were exposed to a boring set of tasks and were then paid to tell other subjects that the experience had been interesting and enjoyable. On a subsequent test of their enjoyment of the tasks, a change was found in the direction of increased expressed enjoyment. But, when the reward offered was high and far beyond that necessary to elicit the behavior, the change in attitude was relatively small.

The two studies cited in the previous paragraphs were based on an instrumental-learning model. Others have considered that classical conditioning, interpreted as a case of contiguity learning, presents a satisfactory model of attitude formation. Staats and Staats (1958) designed an experiment to test the hypothesis that attitude responses could be conditioned to a simultaneously presented verbal stimulus. The procedure involved the presentation of a name, such as "Dutch," which was presented a number of times and accompanied each time by the auditory presentation of a different word. While the words accompanying each name were all different, they all had a common evaluative meaning. For example, the words paired with "Dutch" included the words "gift," "sacred," and "happy;" the words paired with "Swedish" included "bitter," "ugly," and "failure." As a result of this pairing of the names and words, all of which had the same evaluative component, the meaning attached to the names was demonstrated to change.

A similar point of view is stressed in a study by Kimbrough (1956), who proposed that attitudes consist of implicit verbal responses. By this Kimbrough means that when it is said that a person has a favorable attitude toward the law, he has many associations between the concept of law and positive verbal statements about the law and related matters. If, on the contrary, the concept is associated with negative, rejecting statements, then he is said to have a negative attitude. Kimbrough presented some evidence to show how the development of associations might accompany the development of attitudes.

Social Reinforcement of Verbal Behavior

The development of verbal behavior from the point of view of the reinforcement theorist is analogous in many respects to the principles involved in the operant conditioning which occurs in the animal laboratory. Skinner (1953) advocates this point of view in regard to verbal behavior as it may be affected by reinforcement and had developed the thesis in a book on the subject. Ludin (1961) believes that verbal, as well as motor aspects of behavior, are acquired and modified on a reinforcement-of-response basis.

Rheingold, Gewirtz, and Ross (1959) studied the effect of social reinforcements on the vocalizations of institutionalized infants aged approximately three months. The social reinforcement consisted of a broad smile, three "tsk" sounds, and a light touch on the infant's abdomen. The reinforcing stimuli lasted about one second. Reinforcing sessions were given on two consecutive days and in two separate experiments; the frequency of vocalization was found to almost double over the base rate. Extinction of the acquired rate was also demonstrated. Which aspect of the reinforcement was effective cannot be determined from the data. It is a moot point whether this study can be said to involve social reinforcements, for mere stimulus change may have produced the results.

The theory that verbal behavior is modifiable as a result of reinforcement has formed the nucleus for a group of studies of verbal conditioning. The experimental designs of these studies are quite similar. The subtle reinforcement of a designated class of words occurs and a change in verbal behavior is evidenced if an increase in the response rate occurs. Frequently, extinction of the acquired responses by the withdrawal of reinforcements or the counter-conditioning of another word class may be part of the design. Approval which is identified by Skinner and others as a secondary reinforcer, is utilized in the form of a response of "good." Krasner, in 1958, published an extensive review of this literature and summarized the results of the verbal conditioning studies up to that time. Of specific interest for our purpose is the effect derived by saying "good."

Krasner's review lists five studies where "good" proved an effective reinforcer of first person pronouns and two studies where it did not. A further confirmation of the conditionability of this word class is afforded in a study by Buss and Gerjuoy (1958) which finds the emission of first-person pronouns amenable to change by reinforcement. Krasner lists only one previous study where "human responses" were utilized. The results were positive. Similar results were obtained by Metarazzo (1960) in the category of "human responses." Oakes (1960) introduced for the first time social introversion scale items from the MMPI and reinforced subjects for giving answers as scored. He found a tendency for subjects to increase the number of answers given which corresponded with the key.

In only a small number of studies was "good" found to be an ineffective reinforcer. Metarazzo (1960) found the class of "plural nouns" unamenable to verbal conditioning, while in the same study he was successful in conditioning the class of "human responses." As indicated in Table 1, other forms of verbal and physical reinforcement have yielded positive results with plural nouns. Krasner (1958) lists no negative findings with this response class.

It has been suggested that the number of plural nouns which are emitted is large and the nouns vary greatly among classes so that formation of a concept of a class of words is more difficult than in a category of a more limited nature. This statement may apply also to the class of human responses.

Table 4.1 is prepared as a summary of more recent verbal conditioning studies utilizing "good" as the reinforcing stimuli. The preponderance of studies listed in this table, and in the review by Krasner have yielded positive results indicating that modification of verbal behavior is possible as a consequence of verbal approval.

The literature leaves the general impression that the conditionability of a certain class of words is dependent upon the nature of that class. A category of words such as personal pronouns in the first person is a very restricted category and hence is easily conditioned. On the other hand, the category of plural nouns which is very wide in scope is much more difficult to reinforce for frequency of occurrence. The attributes which classify words together as "plural nouns" are not striking and hence this class of words is difficult to condition as a category of behavior. A class of words, to be easily conditionable, must have striking attributes which bind the words together as a class.

Table 4.1

Summary - Recent Verbal Conditioning
Studies Utilizing Approval

Author	Reinforcing Stimuli	Verbal Category Reinforced
Oaks	"good"	Social introversion items-MMPI[a] Reinforced for responding in the scored direction
Metarazzo	"good," "that is good"	Human responses[a]
Buss and Gerjuoy Buss et al.	"good," or poker chips exchanged for candy or cigarette after experiment	"I," "we"[a]
Patterson	"That is fine"	Verbs produced as responses to the Kent-Rosanoff association test[a]
Metarazzo	"good," "that is good"	Plural nouns[b] Human responses[a]
Buss et al.	poker chip not exchanged for candy or cigarette	"I," "we"[b]

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[a] Change in verbal category reinforced in direction hypothesized.

[b] No significant change in reinforced category.

A question often raised in connection with these verbal reinforcement studies is the degree of awareness on the part of the subject of any actions of the experimenter intended to influence the responses of the subject. In other words, can learning occur without the subject's awareness?

Thorndike's (1934) position has been that learning without awareness of what is being learned can and does occur. For behaviorists, such as Skinner, this is an irrelevant issue. Krasner (1958) reviewed 31 verbal conditioning studies where a determination of the awareness of the subject of the conditioning procedure was an essential part of the experimental design. He found no awareness in over half of the studies. In seven specific studies which utilized "good" as the reinforcer, Krasner found some awareness reported in five of the seven specific studies which utilized "good" as the reinforcer.

Metarazzo found a highly significant relationship between the degree to which a class of verbal responses showed conditioning and the degree to which the subjects were aware of the reinforcement process. The results of the Metarazzo study are, in this respect, out of line with those derived from other studies.

Eleven of 26 normal and six of 26 psychiatric subjects in Buss' study (1958b) verbalized an awareness of the experimenter's response to the stimulus class.

Oakes (1960) found no awareness in his subjects. Cohen (1954) reported no awareness among his subjects, yet it is evident that a change in verbal behavior occurred. Cohen counter-conditioned in his study, reinforcing "I" and "we" responses initially and then changing to "he" and "they" responses. The reinforced pronouns increased in frequency of usage during the conditioning series, "I," and "we" being most frequently used during the initial trials, and "he" and "they" increasing in frequency during the counter-conditioning trials.

Some experimenters have discarded the subjects from their studies who have been able to note a relationship of this nature before applying statistical procedures. Nuthmann (1957) found that where the subjects were aware of a relationship between verbal reinforcement and a stimulus class, the behavior during the experiment did not differ significantly from the behavior of those who showed no awareness.

The inconclusiveness as to whether awareness is a factor determining the effectiveness of verbal conditioning stems perhaps from the measures of awareness which have been used. In some instances, the subject who indicates in conversation an insight concerning the relationship between verbal reinforcement and the stimulus class does not show such an awareness in response to a questionaire. There are also degrees of awareness. A subject may be aware of the reinforcers, but not associate them with any particular class of words.

The difficulty of treating data in this area is shown in a case record cited by Krasner (1958). In this particular case, the subject, during the second reinforcement session, remarked that the examiner nodded his head every time he used the word "mother." This was exactly what the experimenter was doing. After this comment, the subject dropped all responses to mother. However, in an interview at the end of 25 conditioning sessions, the same subject denied having noticed anything particular about the experimenter's behavior or of having been influenced in any way.

Adams (1957) also reviewed studies in this area. He attacked them frequently on the basis of methodology and questioned the findings. Postman in his 1947 review considered the topic of learning without awareness should remain an open issue. His later position (1961) based on increasing experimental data seems to favor the learning without awareness thesis, but he states that theoretical and experimental reappraisal are requisites to decision-making in the area. He believes that recognizing the existing contingencies between the stimulus and response increase the probability that a specified response will occur, but that not recognizing the contingencies does not mean the response will not be made. He cites, as an example, the fact that much of our behavior occurs even though all the variables which may affect it are not known.

In a more recent review, Farber (1963) has confirmed the importance of awareness in the determination of response probability. More specifically, he finds that covert behavior is an important determinant of overt responses in verbal conditioning. Siegel et al. (1963) have demonstrated the same sort of relationship in a more traditional academic learning situation; again, the mental processes of the subject at time of learning was significantly related to criterion performance. This matter is of special significance since it is not unusual, especially in studies involving human subjects, to find that the variance within experimental groups is very large in comparison with that which can be attributed to treatment effects. But it is not sufficient to be able to explain the variance; rather, it appears that one of the major problems presently facing research workers is that of discovering the additional determinants (both in terms of variables and their interactions) that contribute to this variability. As the relevant variables are brought under experimental control, they can be manipulated to provide for the optimal conditions for learning.

In comparing the effects of various reinforcers on a response class which has demonstrated conditionability, such as first-person pronouns "I" and "we," Buss et al. (1958) found a poker chip to be an ineffective reward in verbal conditioning for college students and psychiatric patients. Verbal reinforcement ("good") and a reinforcement of a poker chip which had a trade-in value for cigarettes and candy were effective reinforcers, and acquisition and extinction rates were shown to be related to each other in this study. In a later study utilizing subjects from a psychiatric population, Buss and Gerjuoy (1958) reported findings similar to those previously reported (Buss et al. 1958) on the effectiveness of verbal approval and material reinforcers. With psychiatric and neurotic subjects, saying "good" brought an increase in first-person pronouns. Approval is found to be effective with psychiatric subjects in altering verbal behavior; the more tangible reinforcements which may be exchanged for items of value to the individual, serve as reinforcers; while a tangible reinforcement which represents no further personal reward has little value.

Looking back over the studies of the reinforcement of verbal behavior which have been considered here,one is left in doubt concerning the extent to which social phenomena play a central role in such conditioning. The occurrence of events such as a flash of light which have no particular social significance may also produce similar conditioning phenomena. However, the previously cited table by Krasner (1958) suggests that the social factor may play a role. In this table 38 studies are cited in which the behavior of the experimenter was provided as a reinforcing agent and of these 30 produced positive results. On the other hand there are eight studies cited in which a bell or a buzzer or a tone were provided as a reinforcement, and of these only four produced positive results. The studies involving a social element in the reinforcing contingency appear to produce positive results more frequently than do those in which the social element is lacking. There is also the interesting comment made by Krasner that the social reinforcement of verbal behavior occurs best when the reinforcer is a psychologist. This also suggests that the phenomenon is a complex one. Verplanck (1956) observed, in respect to the results of his own conditioning procedure, that it was as if the procedure was most effective if the subject "cared" about the experimenter's behavior. Solley and Long (1958) provide additional data on this point.

In a perceptual learning experiment they found that one experimenter produced learning in the subjects, but a second experimenter did not. The difference in the reinforcing properties of the two experimenters was attributed to the fact that the one who produced learning chatted with the subjects and developed rapport while the other went ahead with the experiment in a businesslike way. Solley and Long suggest that the experimenter who chats with the subjects reduces their anxiety and hence acquires secondary reinforcing properties which produce learning in the subsequent experiment.

Whatever the actual mechanism or mechanisms involved, there is ample evidence that experimenters have differential reinforcing value, and that the difference in value is to a considerable extent dependent upon the manner in which the experimenter interacts with the subject.

Summary

A study of the social conditions which influence the effect of reinforcement is difficult to undertake, mainly because reinforcing effects are confounded with many other variables. For instance, it is commonly assumed that some reinforcements are particularly effective because they result from the behavior of another member of the same species. In terms of this assumption, it would be expected that a reinforcing condition which provided the same amount of information concerning the adequacy of performance, but which did not involve a member of the same species, would be less effective than a socially administered reinforcement. While no study has been found that tackles this problem directly, the interpretation of any such study would not be as straightforward as might appear on the surface; the research considered in this chapter brings out clearly some of the phenomena which complicate the study of social reinforcement.

First, there is the fact that the introduction of another member of the same species into a learning situation may change the situation of the learner, even to the extent of changing the goals, motives, and incentives involved. An adult tutoring a child may find that the goal of the child has become that of interacting with the adult rather than one of learning the task. The effect may be procrastination in order to prolong the social contact or rapid learning in order to please the adult. What will happen will depend upon the motives of the particular child in the particular situation.

Second, social facilitation, a phenomenon which is well established, is dependent upon the specific history of the animal with other members of the species. If one has generally been successful in competitive situations, the introduction of other members that are in competition for the goal will generally result in a more vigorous response. It is perfectly conceivable, given a sufficiently long history of competitive behavior, that competition could also result in more attention being given to the learning task with the result that learning would be attained more quickly and/or more thoroughly. On the other hand, a subject that has not been successful in a competitive situation may withdraw, at least to the extent that the contingent reinforcing stimulus is so improbable that it no longer holds any appreciable stimulus value for the subject. Other specific adjustments, of course, can be made that are not covered in the present review. It would seem that this area would be a profitable one for further research.

The third area of appreciable concern is that of imitative behavior. It is very apparent that in the case of simple responses, one subject can acquire the habit of imitating another on the basis of discrimination learning by making reinforcement contingent upon imitative behavior. This, again, is a variable that has not been fully explored, but which will have to be included in any systematic theory of behavior. It is presently uncertain to what extent it is sufficient for one subject to simply observe the performance of an instrumental response for the purpose of 'learning' the response, but there is increasing evidence that this is of considerable importance, at least with animals higher on the phylogenetic scale. At the present time there is no reason to assume that the results that are accruing are in any way antithetical to a reinforcement theory of learning, for it would appear that the specific mechanism of imitation does not result in an increased habit strength per se, but rather it seems to function as a method for the internal programing of behavior. The specific behaviors that lead to a desired consequence may be verbally (or mentally coded) and may subsequently, through recitation, provide the cues for the behavior. This would be somewhat similar to the shaping of behavior phase of most learning studies; concern is concentrated on the production of the response.

Another area that has not been very adequately explored is that of vicarious experience. It has been demonstrated that if one person is punished, the remaining subjects may be somewhat daunted, but it is not presently clear whether or not reinforcing one subject will in fact facilitate the acquisition of the response on the part of another subject. There is one aspect that is clear. Any reinforcement provides some information concerning the adequacy of the response; for this reason, a person can profit indirectly, in so far as the acquisition of information concerning reinforcement-contingencies is concerned, without necessarily demonstrating an increment in habit-strength. Another aspect of the same problem is the development of some sort of scheme that will enable one to predict the circumstances under which one can profit from vicarious experience. Some data suggest that there is a process of identification involved, at least in some cases.

A fifth factor arising from the review of social stimuli is that of social reinforcers per se. Subjects will respond to simple social contact in such a manner that a response-contingent opportunity to interact socially will serve to reinforce a response. This in part explains the continued popularity of ostracism as a force for social conformity, and is probably closely related to the data relating social need-states, for instance McClelland's achievement motive (McClelland et al., 1953), to learning variables. These conditions, social needs, in turn suffice as the foundation for much of the data that has been accumulated on human verbal and motor conditioning. It has been demonstrated that the personal characteristics of experimenters can be of considerable importance, and the data suggests that rather complex interactions between the characteristics of experimenters and subjects must be assumed to account for the experimental results. To phrase the issue differently, stimuli such as pencil taps, poker chips, head nods and "mmm hmm's," while of demonstrable reinforcing value, are limited in their effect except as they are signs of a social consequence, approval, admiration, attention, etc., or as they serve an informational function, having been paired with "right" or "correct" responses.

In addition to data bearing on the factors that determine what behavior will occur and what stimuli will serve to reinforce the behavior, there has been a little data accumulated on the nature of reinforcable response classes. Most of the studies conducted in the area of learning make use of highly specific responses, such as lever pressing, knob pushing, nose rubbing, or emitting restricted classes of words such as first person pronouns. It is interesting, therefore, to find that general classes of behavior such as expressing opinions are also reinforceable, although little is known about what constitutes a response class.

Chapter 5

Aversive Stimuli as Reinforcers

That living organisms capable of locomotion tend to withdraw from or avoid certain stimuli is an obvious fact, but the causes and consequences of such behavior have long intrigued psychologists engaged in the study of learning. Thorndike throughout his writing referred to such stimuli as annoyers and contrasted them with another class of stimuli which he called satisfiers. The term annoyer does not seem to have survived well in psychological literature and is rarely encountered today. The present preference is for the term aversive stimulus, the introduction of which is generally ascribed to B. F. Skinner. In his early work, Skinner (1938), while not actually referring to aversive stimuli, discussed the aversion which organisms show for certain stimuli; his general work on psychology (1953) makes very extensive use of the term.

An aversive stimulus is one which an organism tends to avoid. Such stimuli cover such a wide range of phenomena as an electric shock applied to the paw of a rat or the word "wrong" announced to a human subject who has just made a response. The study of the influence of such aversive stimuli includes the problem of the effects of punishment on a response and the effect of threat on learning. In short, it involves the influence of aversive stimuli on learning (including the learning to respond to certain stimuli as aversive).

The general class of learning generally subsumed under the heading of aversive stimuli is that of negative reinforcement, but there is more confusion in the field than this simple heading would generally reveal.

Skinner (1953) uses the term negative reinforcement to refer to the effect of an aversive stimulus, the removal of which strengthens a response. An aversive stimulus is generally removed by the organism escaping from the situation and hence the removal of the aversive stimulus may strengthen the escape response. A rat which learns that an electric shock can be terminated by pushing a bar soon learns to push the bar rapidly and forcefully every time the current is turned on. The bar pushing response has been reinforced by the termination of the electric shock.

Bindra (1959), on the other hand, defines a negative reinforcer as an event which decreases the strength of the preceding response. He bases his justification of this definition on the empirical finding (Olds and Milner, 1954; Olds, 1956) that electrical stimulation in several areas in the posterior portion of the subcortical brain depresses the operant rate of response to a lever that controls the delivery of the current. Bindra's definition is,in many respects, very similar to what has normally been regarded as a punishment. In punishment, the incidence of the aversive stimulus is contingent upon the occurrence of a particular response (Mowrer, 1960a).

There is a third situation that can be conceptually distinguished in spite of the fact that it has been traditionally confused with punishment as it is given above. This is the situation in which the removal of a positive stimulus is contingent upon the occurence of a response. For instance, a reward may be withdrawn upon the occurence of a particular response. The assessment of fines, of course, is a common cultural practice.

The three basic models for the action of aversive stimuli and/or punishment are shown clearly in the model below which diagrams the four possible consequences of a response:

	Onset	Termination	Failure to occur when expected
POSITIVE STIMULUS	a	b	c
NEGATIVE (AVERSIVE) STIMULUS	d	e	f

The upper left, a, is the positive reinforcing situation as it has been discussed in the preceding three chapters. The cells, c, d, and e are generally identified as punishment and negative reinforcement respectively, although negative reinforcement for Bindra fits better the contingency identified as d. The cells b and f have received little experimental exploration.

Another matter that will be of great concern as the chapter progresses is that of the exteroceptive stimuli associated with each of the contingencies mentioned above. Consider, as an example, an animal that is placed in an apparatus that, after a minute, provides him with a shock. The shock will generally lead to escape behavior (if escape is possible), and the response that leads to escape will be reinforced. It is unlikely that the animal will wait for the onset of the shock on very many successive occasions. The first trials exemplify *escape behavior*, and the behavior during successive trials on which shock is omitted (by virtue of escape) illustrates *avoidance behavior*. While theoretical interpretations of avoidance behavior differ considerably, there is unanimous agreement that avoidance of a previously neutral stimulus is in some manner dependent upon the stimulus having been associated with an aversive stimulus.

Primary Effects of Punishment

The classic studies of the effects of aversive conditions on learning were undertaken by Thorndike. While in his earlier writings he had held that punishment had the effect of weakening an S-R connection, his final position was that punishment neither strengthened nor weakened such connections (1932, 1935). As Postman (1947) has noted, his announcement concerning his position regarding punishment caused considerable surprise, for there were many studies besides his own on the effects of punishment, and many of these had shown more positive results. Many writers (Woodworth and Schlosberg, 1954; Hilgard, 1956) have subsequently pointed out that Thorndike's conclusion was based upon an interpretation of his data, which in turn was based on an erroneous assumption.

Consider a common type of Thorndike situation in which a list of words is read to a subject who must respond to each word by giving a number from one to five. Suppose, that the experimenter has determined that the correct response to the first word is three, but the subject says five and is punished by the announcement "wrong." Suppose that the subject has a tendency to respond with the number five rather than with any other number. (Most subjects show tendencies of this kind.) The subject will be likely to show the same tendency the next time he goes through the list, and even though punishment weakened this response, he would still be more likely to say five than any other number. Such an experiment would then produce data apparently showing that punished responses tended to recur more frequently than nonpunished responses, but all that actually would be domonstrated was that whatever responses occur tend to recur, and this would be true of nonpunished responses as well as punished responses. Now Thorndike, in interpreting this kind of data, made the assumption that in a five-choice response there was a probability of twenty percent that any response might be given on the initial trial. Then, through learning, Thorndike assumed that correct responses would increase above this twenty percent level in frequency of occurrence and incorrect responses would show a decline. In other words, he assumed that the base rate on any response was twenty percent, but this assumption is an unsound one. If a punished response were to have a base rate at the beginning of the acquisition series of thirty percent and then decline as it was punished to ten percent, the overall response rate of the punished response might be twenty percent, which Thorndike would take to indicate that punishment had not weakened the response. In this particular instance, the response would have been weakened from a thirty percent level to a ten percent level, but the weakening is only evident when the base level is taken at its true value of thirty percent.

Two early studies by Stephens (1934a, 1934b) and a study by Tilton (1939) showed, that when an improved estimate was made of the base rate, punishment in terms of following a response with the announcement "wrong" did produce a weakening of the response. However, there was some agreement that the announcement of "right" strengthened correct responses much more than "wrong" weakened incorrect responses.

Simplified Approaches to the Study of Punishment

Most of the early studies of punishment were undertaken with human subjects and involved tasks involving symbolic content. Some experimenters, notably Skinner and Estes, took the position that the primary effects of punishment should be studied in relatively subhuman subjects with whom some control could be exercised over the condition to which they had been exposed prior to the experiment. In the apparatus developed by Skinner (1938) a typical lever-pressing box was used in which the rat is conditioned to obtain pellets of food. A modification is introduced into the mechanism which makes it possible to administer a sharp slap to the paw of the rat on the pressing of the lever. The slap can be turned on or off at the discretion of the experimenter. In such an apparatus, rats can be conditioned to a given level and experimental procedures can be used to estimate the number of nonreinforced trials which must be undertaken to produce extinction. Skinner asked the question whether slapping the paw of the rat during extinction would result in a reduction in the number of trials needed to extinguish the response. His results, though not entirely clear, point to the conclusion that punishment does not influence the number of nonreinforced responses needed in order to extinguish the response that had been conditioned, but they also suggest that a different conclusion might be reached if severe punishment were to be used.

A systematic effort was made by Estes (1944) to explore the problems raised by Skinner concerning the effects of aversive stimuli on extinction. Estes used a similar apparatus, but used shock as the aversive stimulus. Certain conclusions came out of his work which have stood up well under subsequent scrutiny. First, he found, as did Skinner, that short periods of mild punishment do not reduce the number of unreinforced responses required to produce extinction. Second, when punishment in the form of a mild shock is administered during extinction trials there is a decline in response rate, but this is followed by an increase in response rate once the punishment is stopped. Thirdly, intermittent punishment produces a less marked decline in response rate than does punishment on every occasion. Some evidence indicates that the effects of partial punishment were more prolonged than were those of punishment on every trial. Fourth, prolonged severe shock may reduce the number of trials needed to extinguish a response.

This same sort of response inhibition was demonstrated in a study conducted by Warden and Aylesworth (1927) which preceded some of the most significant work of Thorndike on punishment. These investigators, in the course of a study in discrimination learning, during which an incorrect response was punished with a fairly strong shock, found that the first result was a failure on the part of the rat to respond to either of the alternatives.

The same study also demonstrates an additional consequence that can accrue from the application of punishment, namely, the effect of strengthening responses that are not punished. These investigators demonstrated that on a simple discrimination learning task, rats rewarded for correct responses and punished for errors learned about nine times as fast as a group rewarded only and about twice as fast as a punished group. Thirty years later a similar experiment was run by Brackbill and O'Hara (1958) using kindergarten children. The task involved was a discrimination situation, in which the children had to identify the box containing the candy from its position in the series. Children were either rewarded with candy, or rewarded and also punished by having candy taken away from them when they made a mistake. The condition of punishment alone was avoided since this might involve public relations problems. The results were in accordance with the hypothesis that the condition combining punishment and reward produced more rapid learning than reward alone, a finding consistent with the Warden and Aylesworth results. Brackbill and O'Hara suggest that the effect of punishment in this situation may be to raise the level of drive and thereby indirectly to increase rate of learning.

Numerous other experiments over the last half century have demonstrated that aversive stimuli may indirectly facilitate learning. In a bibliography prepared by Estes (1944) seventeen such studies are listed, and undoubtedly there are others.

How such a facilitating effect may operate is obscure. One common suggestion has been that it operates through producing greater variability in behavior, but this effect of punishment has still not been well established. Thorndike (1949) eventually came to the conclusion that mild punishment in the form of "wrong" or other mild verbal reprimand increased variability. Stone (1950) came to a similar conclusion, as did Estes (1944) while using mild shock in a bar-pressing situation. However, a later study by Stone (1951) in which "wrong" was used as a punishment produced no evidence of an increase in response variability that could be attributed to punishment. Findings of the same sort were produced by Duncan (1960).

What both Skinner and Estes demonstrated was that the primary effect of punishment is that of inhibition. This represented a new formulation of the effect of punishment with considerably different implications than those derived from a theory of punishment which involves the idea that punishment weakens a stimulus-response bond or weakens the strength of a habit. The fact that punishment was demonstrated by Estes to have a quite lasting effect if it were sufficiently severe does not argue against the inhibition theory of punishment. Some inhibitions have great stability and may last for the lifetime of the subject. The inhibition theory of punishment is also consistent with the theory that is widely held that learning is permanent and represents a non-reversible process, and that extinction and related processes are manifestations of the effect of inhibition.

While the work originally undertaken by Skinner (1938) and Estes (1944) with rats indicated that the state of inhibition produced by mild punishment involving either a slight slap on the paw or shock produced inhibitory phenomena which resulted in a slowing down of the rate of bar pressing during the extinction phase. However, the total number of bar presses emitted was not influenced by the fact that the response had been accompanied by mild punishment. As is commonly the case, these results do not seem to be replicated in experiments with human subjects. In one study by Rothberg (1959) using human subjects the task presented involved free associations to a standard list of words with responses involving an antonym as those which potentially involved punishment. The schedules of punishment used involved either 100 percent severe punishment, periodic severe punishment (that is punishment only on some occasions when antonyms were emitted) or mild 100 percent Punishment. The findings were,that during both the punishment and the acquisition series, the subjects emitted fewer antonym responses under the 100 percent severe, periodic severe and 100 percent mild punishment than they did under the periodic mild punishment or nonpunishment or reward procedures. The latter results suggest that the effect of mild punishment may be closely related to the extent to which it conforms to a schedule of 100 percent of the occurring incidents of the particular behavior. The question then is not just a matter of the severity of punishment, but one of the frequency with which punishment occurs. Clinicians have long suggested that it is the relatively small, but persistent nagging punishments which occur day in and day out that have damaging effects on personality. The latter view fits with the experimental data which has just been presented.

Effect of Aversive Properties of a Stimulus On Perception

Studies of perception have not been customarily considered in reviews of reinforcement phenomena, but perceptual responses may be reinforced just as other more directly observable responses may be reinforced. Such studies of perception are of particular interest in that they throw light on the effect of accompnaying aversive stimuli. The general pattern of such perceptual studies is that a stimulus, commonly a black and white figure, is presented to the subject, and at the same time, he experiences some unpleasant stimulus such as an electric shock. The subsequent ability to recognize the figure is then compared with the ability to recognize figures which have not been accompanied by aversive stimuli. A further complication is introduced into these experiments by the procedure of sometimes having the shock under the control of the experimenter and sometimes having it under at least the partial control of the subject. In the latter case the typical procedure is for the subject to be able to turn off the shock by identifying the figure correctly. A variation in this procedure involves the use of ambiguous figures such as the famous one which can be seen either as a vase or a pair of human profiles. In the latter type of study, one of the figures seen is accompanied by punishment, but the other is not. The general procedure is to study the effect of such differential punishment on the frequency with which one or the other figure is seen.

In their review of the many studies in the area Solley and Murphy (1960) point out that studies have been undertaken under a variety of conditions, and there is some difficulty in conjecturing how the often apparently inconsistent results can be fitted together. However, they suggest that a very important condition determining the outcomes of particular experiments is whether a perception becomes a signal for the escape of the subject from the situation or whether it means that he has to endure a noxious stimulus without being able to escape from it. These latter authors conclude that if the presentation of a stimulus is a signal for escape, and the subject can actually escape, that his perception of the stimulus will become clearer and more intense. On the other hand, if the perception of a stimulus is the signal for the onset of a noxious condition such as shock from which there is no escape, then the stimulus is likely to be perceived with less acuity, and the subject may fail to perceive it at all. This makes a considerable degree of sense. There are obvious survival advantages in becoming highly sensitive to those stimuli which warn that some unpleasant state of affairs from which one can escape is likely to occur, but there is little to be gained in recognizing a stimulus warning of an unescapable state of affairs.

Stimuli Associated With Punishment

While the effect of severe punishment seems now well established to be that of relatively permanent inhibition, other effects have also been established. This is especially apparent in a situation that provides the experimental animal with an opportunity to associate the onset of the aversive stimuli with a particular stimulus configuration, for instance with a particular experimental box. When this arrangement is provided, it is possible for the animal to avoid the aversive stimulus by responding to the associated exteroceptive stimuli.

The classic example of a highly stable avoidance response is generally demonstrated by means of a Mowrer-Miller box. This piece of equipment consists of a two-compartment box, the separate compartments of which are divided one from another by a low partition over which the animal can scramble. A gate may be lowered over the latter portion so that the animal cannot climb from the one compartment to the other. The floor of both compartments is fitted with a grid through which the animal may be given a severe electric shock. The general procedure used for demonstrating the phenomenon involves placing the animal in one compartment with the gate lowered so that it cannot move to the other. The gate is then raised, and at the same time, a light is turned on. The animal is then given a strong electric shock, just less than that which will produce a state of tetanus. The animal escapes the shock by scrambling over the barrier. The animal rapidly learns to avoid the shock just as soon as the gate is raised and the light is turned on by moving rapidly to the alternate compartment. Indeed, under some circumstances the learning will occur with a single application of shock and avoidance behavior will continue to occur over many hundreds of trials even though it is never reinforced by the application of any additional trial with shock.

In one such study by Maatsch (1959) eight rats continued to manifest such behavior until a particular criterion of extinction was reached. The number of jumps undertaken by each of the rats was 641, 911, 848, 134, 260, 177, 18, and 28. Extinction is very slow and could be produced by a number of mechanisms including reactive inhibition and interference. If the Maatsch study conditions were arranged so that escape called for considerable effort on the part of the rat this should maximize the effect of work inhibition. Maatsch considered that his data favored an interference theory of extinction in this situation.

Some of the best known studies with this type of apparatus have been undertaken by Soloman and Wynne (1953, 1954) in which dogs were used as the experimental animals. These research workers also reported that the animals continued over long periods with their avoidance performance, even though no additional reinforcement was given in the form of shock. The behavior is highly persistent and Soloman, Kamin, and Wynne reported that it may continue for several hundred trials without showing any sign of extinction. So impressed were Soloman and Wynne (1954) with the stability of the avoidance learning achieved that they suggested that the learning involved may be partially irreversable. Some of the conditions related to the stability of such avoidance responses have been demonstrated. Most of the early investigators of this phenomenon used a shock of such strength that it was just short of producing a condition of tetanus, and there are references in the research papers to the fact that some rats did not survive the procedure.

Later research workers investigated the effect of different levels of shock. For example, Carlson (1960) studied extinction of a traumatic avoidance response in rats using two levels of shock. There was a much greater tendency for the high-shocked animals to resist extinction of the response than the low-shock groups. An essential condition for a response, which is highly resistant to extinction,is the impact of a very strong aversive stimulus. Such avoidance responses also show a certain orderliness with respect to their distribution with respect to time (Black and Carlson, 1959). The effect of the interval between the conditioned and the unconditioned stimulus in traumatic avoidance learning is not clear. Kamin (1954) found that as the interval increased the more easily the response became extinguished, but Church, Brush, and Soloman (1956) found the reverse to be the case. Both of the latter experiments used dogs in a Mowrer-Miller type of apparatus.

Extinction of the Effects of Punishment

The arguments derived from psychology which are commonly used to justify nonpunitive educational systems are that (1) punishment generally has only a temporary effect in reducing the strength of a response and that (2) whatever avoidance behavior is produced is likely to generalize to other aspects of the educational program. The two arguments appear on the surface to be inconsistent, for the one implies that the effects of punishment are transitory and the other suggests that they are permanent. An alternative is, of course, that the direct effects of punishment may be temporary and the indirect effects may be relatively permanent.

It has been found that the immediate effect of punishment is that of inhibiting the punished response. The amount of response inhibition being a function of both the strength of the drive producing the response and the strength of the aversive stimulus employed for punishing the response. It is important to realize, that for a naive subject, the punishment is initially associated with all of the contiguous stimuli, and the response, that of fear, is highly generalizable; the stimuli relevant to the punishment are only slowly discriminated. Mowrer and Lamoreaux (1951) have presented data that is highly pertinent to this point. They placed laboratory rats in a compartment with an electrified grid on the floor. They then sounded a buzzer for five seconds, after which time the animal was shocked. If the rat leaped into the air, the buzzer was terminated and the shock not applied. The buzzer, in short, "signals" the onset of shock, and all other stimuli are irrelevant. During the initial trials the rats made as many or more responses during the intervals between the presentations of the buzzer as they did while the buzzer was actually being sounded. The graph plotting the number of interval responses gradually descended during training, while the number of buzzer responses gradually ascended during the course of training. It would appear, on this basis, that the animals were gradually learning to discriminate the relevant stimulus.

The essential difference between the effects of a very severe punishment and a mild punishment, then, seems to rest on the matter of how far the animal is willing to go in testing reality. It is consistently noted that the animal receives fewer very severe shocks in an avoidance training situation than he does mild shocks. A mild punishment is sufficiently mild that the animal, being motivated to make the response, will learn to discriminate the stimuli that are directly correlated with the punishment.

In training an animal to stay off a bed, it would be far better to severely punish the animal on the occasion of the first offense than to mildly reprimand the animal for each offense. The relevant stimuli are readily discriminated and the animal will learn to stay off the bed while the experimenter is present. Dinsmoor (1955) has pointed out that while the first effects of punishment are inhibitory, these effects wear off and become displaced by escape or avoidance behavior when the situation is such that such behavior can be learned, and when the relevant response can become discriminated.

Avoidance responses which have been developed under conditions of mild shock or other forms of mild punishment have not been demonstrated to manifest any particularly unusual features of extinction. Extinction curves are typically closely similar to those derived from other forms of learning and have been well established since Bekhterev first conducted studies of avoidance phenomena. Responses learned in an avoidance situation show all the usual phenomena of extinction including spontaneous regeneration when a rest follows an extinction series.

Under not all conditions is an avoidance response equally easily extinguished. Miller and Kraeling (1954) shocked rats in a particular alley when they pressed a bar to obtain food. Under such conditions an approach-avoidance conflict is generated in the rat; approach in order to obtain food and avoidance in order to escape the shock. Miller and Kraeling attempted to identify some of the conditions related to recovery from this conflict after the shock was turned off. Recovery will take place if the rats are allowed to remain in the same alley in which they have been shocked. However, a more rapid method of eliminating the conflict is to place the rats in an alley which is distinctively different from the one in which they have been shocked.

Why recovery is easier in a different alley than in the original alley is a matter for speculation. The simplest explanation would appear to be that in the new alley there are fewer cues arousing anxiety than there were in the original alley. This permits the more effective operation of approach tendencies which in turn leads to contact with the bar without shock and with positive reinforcement in the form of food. This train of events leads to the extinction of the avoidance aspect of the response. When placed back in the original box, the additional cues arousing avoidance responses may be sufficiently weak that they offer little competition to approach responses.

In another study by Murray and Berkun (1955) rats recovering from an approach-avoidance conflict in a particular alley turned to an alley (they were free to choose) most different from that in which conflict had been produced. In a later refinement of the study undertaken by Berkun (1957), an approach-avoidance conflict was produced in rats by shocking them while they operated a food-dispensing device in an alley painted black. During the recovery period some of the rats were placed in a new alley, wider than the original and painted grey, while the other half were allowed to recover in the original alley. Rats restrained in the new alley and brought back to the original alley showed fewer signs of avoidance than those restrained in the original alley. Oddly enough, in this study rats originally trained with intermittent reinforcement recovered more quickly than those trained with 100 percent reinforcement.

In a further study of the problem of recovery from conflict by Berkun

(1957) various factors related to the provision of positive reinforcement during the recovery period were explored. The importance of positive reinforcement during the period of recovery was demonstrated. However, intermittent reinforcement was more effective than regular reinforcement.

Generalization of Aversive Conditions

Very little is known about the generalization of the effects of aversive conditions to behavior other than those with which they are closely related in time. While there is much speculation about this matter in educational circles where the view is generally held that aversive conditions associated with learning are alleged to have a generally depressing effect on intellectual performance.

Two types of generalization can occur in respect to learning involving aversive conditions, stimulus generalization and response generalization. Studies of stimulus generalization investigate the extent to which escape or avoidance responses generalize to stimuli other than those to which the response was originally conditioned. Studies of response generalization, on the other hand, investigate the extent to which the response-inhibition which is produced by punishment generalizes to similar responses.

Hearst (1960)conducted a study of stimulus generalization with monkeys. He was interested in comparing the response gradient for situations involving punishment with those involving reward. Subjects learned to press a lever to avoid punishment (shock) and to pull a chain to produce a reward (food), the responses occuring in the presence of light. Later the light was varied and the response frequency at each level of illumination was determined. Hearst found that the curve of stimulus generalization for reward was much steeper than for punishment. He commented that his data showed a virtually undiscriminated avoidance response for the response occurred with equal frequency at almost all levels of illumination of the light. On the other hand, the reward response showed a well discriminated gradient.

The generalization of the inhibiting effect to other responses has hardly been explored. An exception to the latter statement are those studies which have sprung from the work of Maier and Wapner (1944) and which have demonstrated that aversive stimuli accompanying discrimination learning can, under some circumstances, produce highly disorganized behavior.

The only extended program of research with human subjects which has dealt with the problem of generalization of the effects of aversive conditions to other responses is research concerned with the effects of failure. In terms of the classification of aversive conditions previously given, failure is a condition which occurs when a rewarding state of affairs fails to materialize. Whether such an aversive condition produces in human subjects effects comparable to those produced by other aversive conditions is not known at this time.

The experience of failure is generally produced in the laboratory by assigning the subject a task and then evaluating his performance in derogatory terms. In such a situation the feedback may be highly damaging to the ego of the subject. For example, after a college senior has worked long and hard on a task that seems difficult to him, he may be told "Your performance is very disappointing. Most high school graduates do much better than you have done." Numbers of studies have been patterned after this style.

Failure situations represent one category of situations in the general class designated as stress situations. A review of studies up to 1952 has been provided by Lazarus, Deese, and Osler (1952) who make the definitional statement that "stress occurs when a particular situation threatens the attainment of a goal." Thus, their review is concerned largely with those studies in which persons are exposed to a situation in which some motive is thwarted. Thwarting, and the resulting stress is generated either by arranging a situation in which the subject necessarily experiences failure or by arranging working conditions which make the task difficult to accomplish. A common procedure in the latter case is to provide distracting stimuli so that the demands on the subject become greater than he can be expected to handle.

Lazarus et al., conclude in their review, taking all available studies together, that failure generally produces a decrement in performance on verbal tasks. They cite as of special interest the study by Zeller (1950) which demonstrated in a nonsense syllable learning task that the experience of failure resulted in a decrement in both recall and in relearning the task. Zeller claimed that performance steadily declined as failure became accumulative. His suggestion was that repression of material associated with failure was the chief mechanism involved in the phenomenon. While Lazarus et al., cite several studies which support the point of view that failure experience results in a work decrement, there are some studies to the contrary including one by Lazarus and Erikson (1952) which indicated that there were considerable individual differences in responses to failure.

Similar effects on performance in perceptual-motor skills were noted in the studies reviewed by Lazarus et al. The experience of failure generally produced a decrement in performance. Other forms of stress also produced a decline in performance. A number of studies discussed in the review attempted to produce stress by giving subjects insoluble problems to solve prior to the administration of the perceptual-motor performance. The responses of the subjects to stress varied from experiment to experiment and was probably determined partly by the nature of the perceptual-motor test, partly by the nature of the condition producing stress, and partly by the characteristics of the subjects. Two studies are cited which are of special interest in the present connection since they indicate that stress may produce a decline in the ability to perform mediating processes related to problem solving. Of these two studies, one is by Seashore and Bavelas (1942). It provided data showing that scores on a draw-a-man test declined when frustrations were introduced. The other is a well-known study by Barker, Dembo, and Lewin (1941) which demonstrated a deterioration in intellectual performance as a result of stress.

Interpretations of the Effect of Punishment

Once the point had been reached at which considerable data had been collected, psychologists began to speculate concerning the mechanism involved which determines the effect of aversive stimuli.

The common explanation for traumatic avoidance learning is that it involves some subtle form of secondary reinforcement. Mowrer (1950) in his earlier views, and Soloman and Wynne (1953) have taken the position that the shock produces an internal condition of fear which functions as a drive; this drive is subsequently reduced by the avoidance behavior.

In situations in which there is a conditioned stimulus such as a buzzer which is terminated when jumping occurs, then the termination of the buzzer may function as a reinforcer. Studies have demonstrated that visceral responses which are generally assumed to accompany a drive state of fear, such as an increase in heart rate, do occur during traumatic learning and Carlson (1960) has shown that the more resistant the responses are to extinction, the greater the increase in heart rate that accompanies them. The explanation of an intervening variable of fear operating with reinforcement of the escape response by drive reduction seems to be highly plausible. Since fear responses can be generated by a single very painful experience, and since fear responses are well known for their permanency, it is hardly surprising that the reduction of fear produced by jumping the barrier becomes a well reinforced response with considerable permanency.

The main argument against the explanation of traumatic avoidance learning in terms of an intervening variable of fear mediated by means of autonomic responses is that such autonomic responses are slow to develop, so slow in fact that they might hardly appear soon enough after an electric shock to play a part in subsequent behavior. While the response of a skeletal muscle may occur in less than 0.1 second, the response of smooth muscles is typically slow and often involves a latency of several seconds. If avoidance learning involves the association of cues with the emotional state of fear, and if the central element in fear is a set of smooth muscle responses, then this model of learning is plausible in so far as smooth muscle conditioning is possible.

Reinforcement theorists have been divided into two camps with respect to this issue. On the one hand there have been those such as Mowrer in the earlier versions of his theory (1947, 1950) and Skinner (1935) who have taken the position that reinforcements operate only on responses involving the skeletal muscles, other learning theorists, notably Hull (1943) and Miller (1951), have taken the position that reinforcing stimuli have the same effect on smooth muscle responses as they have on the responses of the skeletal musculature. Those who reject the reinforcement theory of smooth muscle learning take the position that only contiguity in time is needed between the unconditioned stimulus and the conditioned stimulus for such learning to occur.

A crucial test of the validity of either of these positions requires that conditioning experiments be run designed to test whether reinforcement is, or is not necessary for the conditioning of smooth muscle responses. In order to conduct such studies a readily available smooth muscle response must be recorded, and one that satisfies the criterion of direct observability is the pupillary reflex which has been extensively studied in relation to conditioning.

Gerall, Sampson and Boslov (1957) who reviewed previous experimental studies related to the conditioning of the pupillary reflex concluded that the results were inconsistent and set out to test whether a reinforcement or a contiguity concept could best be applied to the conditioning of this reflex. They designed an experiment which involved an attempt to condition the responses in human subjects to a 1,000 cycle per second tone under two different conditions. Under one of these conditions the unconditioned stimulus was a light which went off producing pupillary dilation. Under the other condition the unconditioned stimulus was an electric shock which also produces pupil dilation. Under both conditions the cessation of the unconditioned stimulus occurred at the same time as the cessation of the conditioned stimulus, namely the tone. The hypothesis was that the cessation of the shock would function as a reinforcer, and hence the pupillary reflex would become conditioned to the tone. Conversely, it was hypothesized that the turning off of the light had no reinforcing properties, in which case the conditioning of the pupillary reflex was not expected to occur. The results supported the latter position. The condition involving shock produced conditioning of the pupillary response, but the removal of the light did not. The authors of the study concluded that the results were consistent with a reinforcement theory of conditioning smooth muscle responses rather than with a contiguity theory. It is possible, of course, to interpret the results as being consistent with a contiguity theory, and so far, no scientist has been able to design an experiment which will conclusively establish whether learning in a particular situation is best understood in terms of reinforcement or contiguity theory. However, the results do support the Mowrer position on avoidance learning in so far as they demonstrate that cues can come to elicit smooth muscle responses which may be the major behavior manifestation of fear.

Mowrer also points out (1956a) that some persistence in traumatic learning may be due to the generalizations of fears in other situations to the experimental situation itself and cites Whiteis (1956) to support his position. In the Whiteis type of experiment a rat is shocked at the start of a straight alley runway and understandably runs to the other end. Such a response will ordinarily extinguish after a number of nonshocked trials. However, if during the extinction series the animal is given a shock before reaching the other end, the behavior of running to the other end may acquire a great degree of permanence. Mowrer suggests that the reason for this is that the fear produced by the shock near the end of the runway generalizes to the beginning of the runway, which, hence retains its capacity to induce fear and hence escape behavior. The experimental results fit well the experiences of clinicans who have found that fears readily generalize to new situations through the failure of the individual to discriminate the new situation from others which have aroused fear.

Further evidence in support of a fear interpretation of avoidance behavior is provided by the fact that when an animal has been shocked in some particular apparatus, he will exhibit motivational drive characteristics that will support further learning. The earliest experiment on the use of fear as an acquired drive as a basis for the acquisition of new skills was undertaken by Miller (1948) who showed with albino rats that the acquired drive of fear could be used to motivate the learning of skills. Miller, in the introduction of his experimental study, points out that fear has the reputation of being a drive which is easily acquired and which becomes a motive operating in much subsequent behavior. In the classic experiment by Miller, rats learned to escape from an electric shock given in one compartment of a box by escaping into the other. Then, without further shock the animals were trained to turn a wheel to open a door and hence to escape into the other compartment. As a further step in the study, the wheel turning response was extinguished and a bar pressing response learned in its place. The assumption was that the electric shock induced a drive of fear which is later aroused every time the rat is placed in the compartment in which it is shocked. Escape results in reduction of the fear which reinforces the responses which led to escape.

Because an intervening concept of fear seems to play such a central role in the more plausible interpretations of the effects of punishment and avoidance conditioning, there is good reason for interpreting the findings in terms of Mowrer's recent revision of his two-factor theory.

Mowrer's Extension of the Inhibition Theory of Punishment

Mowrer has long been known for his developments in theory of learning, and his revised version of the two factor theory (1960a, 1960b) has much to say concerning the manner in which punishment operates. In order to understand his position with respect to punishment a brief outline of the general theory of learning which he presents must first be outlined. The writers feel apologetic about the brevity of the outline presented here in view of the elaborate exposition which Mowrer considered necessary to state his case. However, the brief outline presented here seems to cover the essential features of the theory needed to understand his theory of punishment.

According to the Mowrer position, the onset of any primary drive results in an internal emotional state which he refers to as fear. When the primary drive is reduced, another internal emotional state is produced which, for the lack of a better name, is referred to as hope. The development of skills are related to drive reduction and the operation of hope while avoidance behavior is a product of drive increase and the occurrence of fear. Mowrer distinguished between what he calls primary and secondary punishment. Primary punishment is the product of stimuli which we associated with drive onset. Some of these stimuli are those which have been associated with the onset of pain, but the onset of any primary drive may become associated with stimuli which in turn become punishing stimuli. These stimuli, through learning, come to produce secondary punishment.

Mowrer distinguishes not only between primary and secondary punishment, but also divides secondary punishment into two categories which he refers to as Type 1 and Type 2. These types of punishment he defines as follows:

> Type 1 secondary punishment,--This occurs when an action is followed, not by primary punishment, but by a warning. This results in an increase in the level of fear.
>
> Type 2 secondary punishment,--This occurs when there is the withdrawal of the expectation of a reward. The reaction is one of disappointment and involves a reduction of hope.

These forms of secondary punishment are considered to be the opposite of what Mowrer refers to as Type 1 and Type 2 secondary reinforcement. Type 1 secondary reinforcement occurs on the termination of a warning that a punishing state of affairs is to occur. Type 2 secondary reinforcement occurs when a signal indicates that a reward is to be expected.

If a response occurs and a punishing stimulus impinges on the responding organism, according to Mowrer, two trains of events may ensue in this situation. These are as follows:

> 1. Stimuli occurring within the body at approximately the same time as the punishing event come to produce an inhibition when these stimuli recur on some future occasion of that response. This is the inhibiting effect of severs punishment. This is referred to by Mowrer as passive avoidance learning. The organism learns to avoid making a response because it is punished.
>
> 2. External stimuli occurring at approximately the same time as the punishing event tend on future occasions to arouse fear which leads to escape behavior and later avoidance behavior. This is referred to as active avoidance learning. The organism learns to do something to avoid a punishing event.

The argument of Mowrer is a persuasive one and his distinction between passive and active avoidance learning is probably useful. The latter two categories can probably be more easily distinguished on a theoretical than on an experimental level. A punishing event will almost inevitably become associated both with external and internal stimuli and hence, if the conception is sound, result in both an internal set of inhibitions and a tendency to manifest escape behavior.

For Mowrer the overall function of punishment is the opposite of the functioning of reward. While the immediate response conditioned to punishment is fear, the immediate response conditioned to reward is referred to as "hope." The latter term is a label rather than an attempt to provide a scientific and precise description of what it involves. The arousal of fear or hope produce internal stimulation to which other activities may be conditioned. Increases in primary drives result in fear conditioning and result in escape or avoidance behavior. On the other hand drive reduction such as is produced by food given to a hungry animal conditions a response of "hope" to related stimuli and the reinforcement is a drive reduction process. Fear conditioning is associated with a drive increase (incremental) and hope conditioning results from drive reduction (decremental).

Mowrer summarizes his two factor theory of learning in the following table:

	Decremental (+) Reinforcement	Incremental (-) Reinforcement
Response Correlated Stimuli (sensations)	Response Facilitation ("habit")	Response Inhibition (punishment)
Independent Stimuli	Place Approach Behavior	Place Avoidance Behavior

(From Mowrer p. 25, 1960b. The word "sensation" has been added.)

The Mowrer hypothesis (1960a) that punishment or a nonavoidance threat situation produces internal inhibition while in an avoidance situation there is powerful motivation of the escape responses finds support from experimental studies. A study of Silverman and Blitz (1956) is particularly relevant in this connection. In this study, subjects undertook a serial learning task under conditions of no threat, a threat of shock which the subjects believed might be avoided and the threat of unavoidable shock. The instructions for the latter two groups were simply "During the experiment, you *may* receive a shock," and "During the experiment, you *will* receive a shock." The directions thus do not link the incidence or avoidance of the shock to quality of performance on the task. The writers here assume that the subjects linked the probability of receiving a shock to performance on the task. While it is almost inconceivable that they would not, there is no evidence in the research report that they did. In the Silverman and Blitz study all subjects were administered the Taylor Manifest Anxiety Scale and from the initial group of subjects was selected a high and a low anxiety group. The findings were that subjects who obtained high scores on the Taylor Manifest Anxiety Scale showed no significant response to threat either in an avoidance or a nonavoidance situation. In contrast, the low scoring group on the Taylor scale showed impairment by nonavoidance threat and improvement with avoidance threat. Presumably, subjects who scored high on the Taylor scale showed a high level of fear or anxiety at all times and hence threat did not change the level of fear and result in a modification of behavior.

A long standing argument against the position which Mowrer advocates is that secondary reinforcers produced in the laboratory tend to have transitory effects. This is to be contrasted with the extraordinary permanence of avoidance responses which has long been a matter of comment by clinical psychologists who have wrestled with the problem of eliminating nonadaptive avoidance behavior and the anxiety related to it. Psychologists have also been able to duplicate some avoidance behavior in the laboratory and through the use of the familiar Mowrer-Miller box have been able to demonstrate avoidance responses which will take place for hundreds of trials without extinction and without any apparent reinforcement operating. Much controversy has existed concerning the nature of the secondary reinforcements operating which keep such behavior from being extinguished. Presumably, secondary reinforcements are operating, but again the question arises why the secondary reinforcers are not rapidly extinguished.

The latter question is an important one to answer. Pavlov found that second order conditioning was difficult to establish because the power of the secondary reinforcers became rapidly extinguished. Pavlov gave up attempts to produce third order conditioning. The facts thus discovered have long seemed inconsistent with much observed behavior which at least appears to demonstrate that some secondary reinforcers have very high resistance to extinction, not only in the case of avoidance behavior which has been mentioned, but also in many aspects of behavior involving positive rewards and approach rather than avoidance.

The results achieved in studies of second order and third order conditioning have been parallelled in studies of instrumental learning. In the classic study of Wolfe (1936) chimpanzees were taught to work for tokens which in turn could be used as secondary reinforcers. Hull (1943, 1951) cites numerous studies in which objects have acquired secondary reinforcing properties, but these have been mainly objects which have acquired positive reinforcing properties, and most knowledge of secondary reinforcements have come from the study of such phenomena. It is for this reason that these studies are under consideration in this section despite the fact that it is concerned primarily with avoidance learning. Generally the studies show that the secondary reinforcing qualities acquired by a stimulus have only transitory value, though not all have observed this effect. Mowrer (1960a, p. 120) points out that Eccher and Culler (1941) found in a relatively early experiment on secondary reinforcement that the secondary reinforcing property of a stimulus might be quite stable in its effect without further pairing with the original stimulus through which it had derived its properties. An important problem is that of determining the conditions under which secondary reinforcements can acquire stable properties. In recent years some experiments have been undertaken to determine these conditions and particularly notable have been the studies of Zimmerman.

In the Zimmerman procedure (1957, 1959) the essential feature is that a training plan is set up in which a schedule of partial reinforcement is established in order to develop secondary reinforcing properties for a stimulus. Thus in his first experiment (see Mowrer, 1960a, p. 114-115) Zimmerman was able to develop secondary reinforcing properties for a buzzer. In the latter experiment a thirsty rat was placed in a box fitted with a water delivery mechanism. After the rat had become habituated to the mechanism and had drunk a number of times a buzzer was connected which emitted a sound just prior to the operation of the water delivery mechanism. The schedule was then modified so that water delivery did not always follow sound of the buzzer. Ultimately, water was delivered only on about one occasion in fourteen when the buzzer was sounded. When this state had been reached the buzzer had acquired highly stable properties as a reinforcer and could be used to produce subsequent learning.

In a later experiment using a more complicated conditioning procedure (1957) Zimmerman was able to produce an even more stable secondary reinforcer. The strength of this reinforcer was evident in the fact that it was able to generate in a rat over 2,000 bar pressing responses with the use of this secondary reinforcer.

The Paradox of Stimuli Having Aversive Properties Functioning as Positive Reinforcers

Theories of the operation of aversive stimuli have been commonly criticized on the grounds that there are occasions when a stimulus which usually produces avoidance behavior or inhibiting effects may appear to function as a positive reinforcer. The phenomenon under discussion was originally demonstrated by Tolman et al. (1932) in a provocative article which claimed to throw doubt on the validity of the law of effect. In this demonstration human subjects were required to find their way through a maze-like arrangement and a bell or a shock or both were used to inform the subject of the correctness of his choices. Tolman and his associates found that (p. 605-606) "the bell-right and the bell-shock-right curves are better (not worse) than the bell-wrong and the bell-shock-wrong curves." These results were interpreted as providing evidence contrary to the law of effect. The conclusion drawn from the experimental results was that the data did not conform to what would be expected in terms of the law of effect and Tolman and his associates proposed a set of three laws as substitutes. While Tolman was apparently impressed by the apparent paradox of a stimulus which is normally aversive functioning as a positive reinforcer, recent writers such as Dinsmoor (1955) have pointed out that there is really no paradox at all: An aversive stimulus has many properties, only one of which is that of producing negative affect.

Aversive stimuli also carry information which may be used to guide behavior apart from any accompanying aversive properties they may have. These two properties may have independent effects on behavior and the result for the one may completely overpower the effect of the other.

Some effects of aversive stimuli other than those which lead to avoidance or withdrawal have been explored by Muenzinger and his students (1934, 1936, 1937, 1938, 1951, 1952). His general conclusions are that shock introduced in the course of a learning and at the choice point, with a resulting pause in the animal's behavior, results in the facilitation of learning because the shock makes for increased sensitivity to the cues in the situation. This perceptual theory has high plausibility but it does not account for some of the results achieved by other experimenters. In an old experiment by Drew (1938) rats were shocked in the goal box which was sufficiently removed from the choice point that choice-point behavior could hardly be influenced by it, but the shock nevertheless had a facilitating effect on learning.

At one time Mowrer (1950) attempted to account for the Muenzinger results in terms of a drive reduction theory of learning. The essential feature of this explanation was that the shock produced an internal drive state referred to as anxiwty which was then reduced as the rat proceeded beyond the point where he was shocked. The drive reduction interpretation fails to explain the results of those experiments in which the shock is administered, not at the choice point, but in the goal box. Such results have been reported by Drew (1938) and Muenzinger (1951) who also noted that facilitation occurred only if the rats had been previously exposed to a procedure adapting them to shock. While Wischner (1947) and others believed that the effect of shock was dependent upon whether the animal could or could not retract its steps to correct errors, this condition does not seem to be an important one in producing the facilitation effect.

Prince (1956) noted that data existed supporting both a perceptual sensitivity and a drive reduction theory of shock and attempted to design experiments which would settle the controversy. In one experiment he ran rats in a black-white discrimination procedure and shocked both wrong and right responses immediately after the choice point. Under these circumstances a perceptual facilitation procedure predicts that facilitation of learning would occur, but drive reduction theory predicts no such facilitation since anxiety reduction should stregthen both right and wrong responses. The results were consistent with a drive reduction theory, but the reason for the lack of the facilitating effect of shock may be that the rats were not habituated to the shock treatment. However, the results of a second and somewhat different experiment supported a perceptual sensitivity interpretation. Thus the Prince paper adds to the inconsistent data available rather than settles the issue. The final chapter of this report provides additional discussion concerning form of feedback in relation to learning.

While aversive stimuli provided by electric shock may provide information, and perhaps increase perceptual sensitivity and learning related to drive-reduction, they may also sometimes produce a source of stimulation actively sought by stimulus deprived organisms. For example, in a study by Coppock (1954) rats were held in position by a form which encased the body but left the head free. From time to time the rats were given a shock when the head was turned in a particular position. The shock was either preceeded or accompanied by a blinking light. After the conditioning period the rats showed a tendency to turn their heads to the critical position that triggered the light and shock.

One explanation of the Coppock finding is that the light functioned, not as a warning signal, but as a reinforcement. A rat held in a plaster form which restricted movements of the body might be regarded as being deprived of stimulation. In many ways the rat is in much the same predicament as Harlow's monkeys which were confined to a box and which learned tasks when the only reinforcement was the opening of a door providing a window through which they could peer. The rats confined in the Coppock experiment could increase the level of stimulation only by turning the head, an action which would turn on the blinkling light and the shock. A similar performance was manifested by the child in the Solley and Engel (1960) perception experiment who was apparently reinforced by having candy taken away from him because this led to further contact with the experimenter.

Finally, mention must be made of the possibility that electric shock may have effects on the reticular system and raise the general level of arousal of the organism. The results of experiments which have been interpreted in terms of an increase in perceptual sensitivity could well ascribe the effect to an increase in the arousal level which, in turn, may increase responsiveness to specific cues.

The hypothesis that shock raises the level of arousal and hence may facilitate learning is supported by a number of studies. This is a concept similar to that involved in the perceptual sensitivity theory of the positive effects of punishment. Siegel and Siegel (1949) demonstrated that thirsty rats increased their intake of water if they were shocked before being placed in the cage in which they received water. Ansel and Maltzman (1950) found similar results. A related finding was reported by Siegel and Brantley (1951) who used food ard hungry rats in place of water with thirsty rats. In a further study by Ducharme and Belanger (1961) rats were trained to press a lever for water reinforcement. The rats were given an electric shock prior to being placed in the Skinner-type box equipped with a lever and water reinforcement mechanism. Ducharme and Belanger showed that the amount of lever pressing behavior was directly related to the strength of the shock administered.

Summary

1. The terms aversive stimulus and aversive situation have not yet acquired any uniform usage in psychology. Positive stimuli which either produce positive reinforcement, or negative stimuli which produce either a reduced response rate or avoidance behavior, have been studied in relation to learning in three different conditions; these conditions are onset, termination or failure to occur.

2. The early studies of the effects of punishment conducted by Thorndike on human subjects produced equivocal results. Thorndike interpreted them to mean that an aversive stimulus following a response did not weaken the response tendency, but later workers have shown that such a conclusion is not justified if a correction is made for the base response rate.

3. Subsequent work undertaken with rats indicated that the immediate effect of an aversive stimulus following a response was a tendency for

the response to be inhibited. With a mild electric shock the number of trials to extinction is not reduced but with a strong shock it is. The latter may produce either relatively permanent inhibitions or relatively unextinguishable escape behavior. However, in the case of human subjects it is not clear whether frequency or severity of punishment is the most important variable.

4. Another well-established phenomenon is that the punishment of a response may strengthen very rapidly and markedly another which provides either avoidance or escape from the punishing condition. How this effect is produced is obscure at the present time.

5. Aversive stimuli have effects on perceptual processes. A stimulus can become more or less easily perceived depending upon whether it is a signal for an inescapable aversive stimulus or the signal that an avoidance response is in order.

6. Stimuli which initially lack noxious properties can come to produce avoidance behavior when they have been paired with strong aversive stimuli from which a successful escape response has been made. The Mowrer-Miller box provides one of the most dramatic demonstrations of this phenomenon.

7. The immediate effect of punishment is for the response to it to be associated with contiguous stimuli and for the response of fear to be highly generalized. While the initial response to an aversive stimulus is inhibition, as the relevant stimuli become discriminated, the inhibition response may become displaced by an escape or an avoidance response. The extinction of an avoidance response occurs more readily when the organism is placed in an environment which differs substantially from

that in which the response was originally acquired.

8. Some evidence exists that the effects of aversive stimuli may sometimes show a high degree of generalization and affect functions and responses very different from those with which they were originally paired. Knowledge with respect to such phenomena are limited, particularly with respect to the extent to which they are transitory.

9. Interpretations of avoidance behavior and escape behavior have typically introduced an intervening drive state of fear or anxiety which are generally considered to be closely coupled with smooth muscle responses resulting from activity of the autonomic nervous system. While there is evidence that the conditioning of such smooth muscle responses may occur and that such responses are related to the development of avoidance behavior, difficulties are introduced into this kind of explanatory concept by the long latent period of such responses.

10. The most elaborate attempt to account for behavior related to the incidence of avoidance behavior is found in the recent work of Mowrer (1960a, 1960b). He distinguishes between active and passive avoidance learning, primary and secondary punishment, and then draws a further distinction between type 1 and type 2 secondary punishment. Mowrer attempts to distinguish between the effects which come to be produced by external stimuli occurring at the time of a punishing event and those produced by concurrent internal stimuli. Mowrer also presents evidence that under some conditions the secondary reinforcing properties of stimuli may be highly resistant to extinction, a fact which would be highly important for his theory in accounting for clinical manifestations of anxiety in terms of his theory.

11. Some research on aversive stimuli has been held up as providing evidence contrary to the law of effect or its modern descendants. In such studies, the behavior of the learner has been influenced more by the informational properties of the aversive stimuli or by properties other than those which typically produce avoidance or escape. Such studies demonstrate that aversive stimuli may be used to provide information, increase sensitivity to perceptual discriminations, provide a sought-after source of stimulus input in stimulus deprived animals, and raise the arousal level. Such properties are distinct in their operation from the aversive properties.

Chapter 6

Drive Level in Relation to Reinforcement

The relationship of drive to reinforcement is particularly complex. The elucidation of this relationship is made difficult by the confusion that often surrounds the concept of drive. The Hullian definition, in which drive is defined as a function of the time of deprivation $[D = f\ (Td)]$, while appealing in its simplicity, has led many investigators to assume that drive is the only consequent of deprivation; this in turn has resulted in the confusion of the motivational consequences of deprivation operations with the remaining conditions that ensue from these operations. Further, the concept of _generalized drive_ that has more recently gained in popularity has liberated _drive_ from its empirical mooring, and has again opened the door to further confusion. In an effort to avoid some of the ambiguities that have occurred in the literature, an effort will be made to tie results reported in the literature to the variables that have been experimentally controlled.

Cue Function of Conditions of Deprivation.

The fact that the creatures of the world demonstrate a capacity to behave appropriately, their behavior often serving to terminate a condition of deprivation, strongly suggests that the typical animal is in some manner sensitive to its needs. Certainly on an introspective level, few things seem so simple and clear-cut. When we are thirsty, we drink; when hungry, we eat. It is tempting to attribute the same capacity to every creature, But psychologists have yet to concede a homunculus that takes cognizance of our state of affairs and directs our behavior accordingly. On the contrary, most seem bent on the development of a scheme based on concepts with observable correlates. This necessitates the development of the issue in terms of the discriminability of stimulus conditions that might arise as a correlate of condition of deprivation. In terms of emperical evidence, it is necessary to show a difference in response when only the interoceptive stimuli resulting from differing conditions of deprivation are permitted to vary, the exteroceptive stimuli remaining constant. If the external environment is the same on successive occasions, then any difference in response will necessarily be due to some sort of difference in internal stimulation.

In an effort to test the discrimination of conditions of deprivation, Hull (1933) designed an experiment that paired hunger and thirst. Using a fairly simple maze that afforded two paths to the goal box, he asked that they learn to take one route when hungry, the alternative route when thirsty. At least he arranged the conditions so that if the rat had been deprived of food, route A led to food while route B led to a blind alley. If the rat had been deprived of water, route B would lead to water while route A led to a blind alley. With this arrangement, Hull had only to observe behavior at the choice points to see whether or not the rats behaved appropriately. The results are perhaps more a monument to the persistence of the experimenter than to the ability of the rats, for it was fully a half-year before the rats were performing correctly as much as 80 per cent of the time. But the important point is that they did learn to behave differentially. Leeper (1935), using a modification of the design that provided for separate goal boxes, obtained the same results, but with fewer trials.

Should efficiency ever be at issue, it ought to be mentioned that although subsequent generations of rats do not necessarily show improvement, experimenters do. Bolles and Petrinovitch (1954) designed a two-arm maze that provided the rats with living quarters at the choice point. The end of one alley was painted black and the other white, a procedure which maximized the differences between the stimuli associated with the two arms of the maze. Animals learned one end to be the food end and the other end the water end. Further, since it is generally recognized that, at least for rats and debauchers, eating and drinking are not uncorrelated activities, they used the Verplanck-Hayes (1953) method of producing strong and pure thirst and hunger drives. This method, rather than that which relies on the deprivation of a consumable such as food, depends on the consequences of satiating the animal on the alternate commodity. Thus when water is withheld, the animal eats less and presumably experiences stimuli associated with the absence of both food and water. If water is then given, the stimuli associated with an absence of water are eliminated, leaving only the stimuli associated with an absence of food. With this method, Bolles and Petrinovitch had their rats choosing the correct alley to either food or water after only 17 trials. The experiment was conducted on a one-trial-a-day basis, and thirst and hunger drive conditions were alternated day by day. The tendency for rats to alternate spontaneously has been covered in another section of this report, and the design of the Bolles and Petrinovitch study would have provided reinforcement for alternation behavior. The experimenters consider that the alternation phenomenon does not account for their results, for they altered the sequence for each animal prior to the attainment of criterion and saw no evidence of alternation. Further support for their position is afforded by the fact that spontaneous alternation diminishes rapidly as a function of time, and little if any would be expected to occur after a period of 24 hours.

The experiments covered to this point demonstrate that internal stimuli can serve as controls over behavior, and it appears that the response is conditioned to the entire pattern of internal stimulation at the time of the response. Hence the differences between the results obtained by Hull on the one hand and Bolles and Petrinovitch on the other might be in part attributed to the fact that Hull's procedure would not maximize the differences between the patterns of internal stimulation associated with the two conditions of deprivation, whereas the Bolles and Petrinovitch procedure would do this by minimizing the stimuli associated with the opposing need. On the other hand, Kendler (1946) presents some data that suggest that the relationship is not so simple. He gave 20 rats four trials daily for seven days while they were simultaneously hungry and thirsty after they had been deprived of food and water for 22 hours. During the training trials, the rats found food on one side of the T maze and water on the other, but the internal pattern of stimulation remained the same. Hence both responses, the turn to water and the turn to food, were made with the same pattern of internal stimulation resulting from deprivation. On test trials, given one daily for four days, the rats responded appropriately when either hungry or thirsty. Since the patterns of internal stimulation were the same during training, a process other than that of conditioning a response to a pattern of internal stimulation (resulting from a condition of deprivation) must be introduced to account for the differential responses. Kendler suggests that the results can be explained by either assuming that the response is conditioned only by those internal stimuli that are reduced by the goal response, or by assuming that the response is also conditioned to stimuli that are produced by anticipatory goal responses. Either of these explanations would provide differential conditions of stimulation that would account for a difference in response during the test trials.

Kendler, in a later study with Law (1950), conducted a further study to test the hypothesis that only those drive stimuli which are themselves reduced become connected to a rewarded response. In this experiment, animals which were either hungry or thirsty were trained to run to food in one arm of a T maze. In a second task in which the same rats were all thirsty, they were trained to run to water in the opposite arm. If the stimuli connected with the thirst drive had become tied to the food getting sequence of responses, then the rats that had learned to run to food while thirsty should have difficulty in learning to run while thirsty to water placed in the opposite arm of the T maze. This did not happen and hence the data were consistent with the hypothesis.

Levine (1956) has also made a direct test of Kendler's hypothesis. His working hypothesis also was that drive stimuli not reduced by a goal response will not become conditioned to the rewarded response. In order to test the hypothesis, he used two groups of rats both of which had been deprived of food for 22 hours prior to the training trials, and trained them to go to one side of a T maze for food. In addition to the withholding of food, one of the groups was also deprived of water so that for this group the response was learned in the presence of stimuli associated with the absence of both commodities. Following the initial problem, he then put both groups on a 22 hour water deprivation schedule and trained them to find water in the opposite goal box. If the stimuli associated with thirst had not become attached to the food-finding response, the groups would be expected to perform equally well. On the other hand, if the stimuli associated with the lack of water were conditioned to the previously learned response to the other arm of the maze, then this would be expected to reveal itself by exercising a decremental effect on the learning of the new habit. This is what happened. The rats trained under both hunger and thirst for the first problem learned the new habit significantly more slowly than the rats that had learned both habits only under relevant conditions of deprivation. He conducted another experiment that counterbalanced the order of the needs in the first (learning first to find water and then to find food) and obtained the same results. In view of these conflicting results of Levine (1956) and Kendler and Law (1950), the status of the Kendler hypothesis is in doubt.

To the extent that the two experiments are comparable, more credence can be placed in the findings obtained by Levine, for Kendler and Law were in the position of accepting the null hypothesis, while Levine rejected the null hypothesis on two occasions at an acceptable level of confidence. It may be, of course, that a response is initially conditioned to the entire pattern of stimulation at the time of training, but that the relevant stimuli are gradually discriminated, thus irrelevant stimuli no longer elicit the response; during this time the animal also begins to make fractional antedating goal responses that provide further for discriminative responses. There seems no reason at present to elect one or the other of the two hypotheses.

The hypothesis that the stimuli that serve to elicit the response don't have to be reduced by the goal response has received further confirmation by Levine (1953). Indeed, his results strongly suggest that the stimuli arising from a condition of deprivation can serve as an independent cue for the correct response, no matter what the relationship to the reinforcement. Capitalizing upon the propensity on the part of rats to avoid bright light, he inserted a strong light in an experimental chamber and arranged two panels, either of which could be wired to turn off the light. The chamber was designed to restrict the movement of the rats and to keep them in a position facing the panels which were positioned side by side. The one on the left was white and the one on the right was black so the rat could discriminate either color or place or both. On days that the animals had been deprived of food they could turn off the light by pressing one panel. On the days they had been deprived of water, the control of the light was relegated to the other panel. After 450 trials, 10 of 11 rats had learned the task to criterion and were exercising effective control over the light.

Additional evidence has accrued to support the ability of rats to utilize internal stimuli as cues for responding. Ansel (1949) conducted an elaborate experiment that also employed hunger and thirst stimuli as discriminative stimuli, but in this case to guide the animal in responding to escape from fear or anxiety that had been conditioned to the apparatus by shocking the animal therein. Again, one side would lead to escape when the animals had been deprived of food, the other when the animals had been deprived of water. After extensive trials, two animals reached the criterion set by the experimenter. To the extent that the experiment afforded an adequate test of learning, the results also argue against Kendler's selective association hypothesis. The fact that so few animals reached criterion in Ansel's study stimulated Levine to conduct the experiment reported above. Ansel also replicated his experiment, but with the correct response leading also to the substance that had been denied the animal and obtained much more rapid learning. Ansel interpreted these results as confirming the importance of fractional anticipatory goal responses, since the overt responses had presumably been reinforced in the previous experiment.

Bailey (1955) expanded upon Levine's design in a further test of the ability of animals to respond differentially to internal cues of hunger and thirst. He was also interested in the facility with which this discrimination could be made in contrast to a discrimination involving the presence or absence of a tone which was of either long or short duration. Bailey first used a 150-watt light as the noxious stimulation which the rat could turn off by pressing the correct panel. One panel would turn off the light if the animal were hungry and the other would turn it off if the animal were thirsty. The panels were also hooked up with a tone, and pressure on one panel would turn off the light in the presence of the tone and pressure on the other in the absence of the tone. Learning was difficult. Indeed, it soon became apparent to Bailey that his animals were not learning the task and he found it necessary to modify his experiment. He changed from a 150-watt to a 300-watt light as the source of the "drive stimuli," and punished incorrect responses by incorporating a photo flood lamp which turned on concomitantly with an incorrect response. He also ensured that each day terminated with a correct response. Under these conditions, four of the five experimental groups eventually demonstrated a reliable difference in their responses to the two panels, selecting the appropriate panel a significantly higher proportion of the time.

Bailey found that the rats, on the average, learned equally well to thirst cues, hunger cues, and the cues provided by the tone of short duration. In the case of the tone of longer duration, learning did not occur. Why the sound should not be effective as a cue-source when its duration in the learning situation is about as long as the hunger and thirst cues is not explained by Bailey.

Behavioral Propensities and Deprivation

Petrinovitch and Bolles (1954) have reported an additional study with intriguing implications. We have seen that the internal stimuli associated with various conditions of deprivation can, in addition to motivating an animal, serve as cues for responses, but the latter scientists have gone further to demonstrate that different conditions of deprivation might facilitate different types of learning. They used both animals that had been deprived of food and animals that had been deprived of water for 23 hours. The animals were divided into two groups, those that were to learn a response alternation habit, and those that were to learn a fixed response. Their results showed that thirsty animals were superior to hungry animals in learning a fixed location of a reinforcing substance, but that the hungry animals were superior to the thirsty animals in learning a response alternation habit. Perhaps more telling is the fact that the hungry animals ran faster and made about as many correct responses on the first and third of the three daily trials, but made more than twice as many errors on the second trial when learning the fixed location of food. This difference is significant at the .001 level of confidence. They suggest that the physiological conditions that accompany food deprivation facilitate variability of behavior while those accompanying water deprivation facilitate stereotyped responses. This sort of mechanism would make sense in terms of evolutionary development, for water is usually found in a fixed location whereas animals typically have to forage for food.

In addition to being able to discriminate between the stimuli correlated with different kinds of deprivation, Jenkins and Hanratty (1949) have shown that rats can utilize the stimuli associated with different levels of deprivation as cues for different responses. Having been trained to find food in a T maze, rats were faced with the problem of turning to one side of the maze when they had been deprived of food for 11 1/2 hours, but to the other side if they had been deprived of food for 47 1/2 hours. Since the maze stimuli remained invariate from one trial to the next, it is argued that the only stimuli available that would provide for a differential response are those correlated with the different periods of deprivation. Hence a differential response would be evidence for the discriminability of the stimuli. The reasoning seems sound. Jenkins and Hanratty conducted trials for 50 days and performed their analysis on the trials occurring during the last 25 days. Assuming that the rats would perform at a chance level (i.e. that the rats would make the correct turn on 50 per cent of the trials) if there were no learning, they tested the significance of the level of performance and found that the probability of their results having been obtained had there been no learning was less than .001.

The data revealed an increasing monotonic trend to respond correctly with greater frequency as a result of an increased number of trials. By the fiftieth day the rats were responding correctly about 85 per cent of the time. Essentially the same results were obtained in an analysis of the trend for the first trial following each change in deprivation, an analysis the authors felt to be more critical.

Bloomberg and Webb (1949) independently conducted essentially the same sort of experiment, and they labored just as long to obtain their results. After appropriate preliminary handling and training, 12 rats, 10 male and two female, were given training trials in a T maze, a turn to the left side being reinforced when the rats were 22 hours hungry and a turn to the right side being reinforced when the rats were only three hours hungry. Following the initial free-choice trial, subsequent trials were forced as necessary to insure equal practice in running to the reinforced and unreinforced sides of the maze. Only one of the twelve rats had attained the criterion of learning which was six successive correct responses on the free trials following a change in the level of motivation. Discouraged in the procedure but perhaps encouraged by the results of the one rat that had achieved the criterion level of performance, they modified their design, procedure and apparatus. Following each free trial, they forced subsequent trials so that there were five reinforced trials and only two nonreinforced trials. They added a door at the choice point and increased the amount of food given during the daily feeding. With these conditions, they conducted trials for an additional eight weeks with two male and 10 female rats. After 185 reinforcements of each response, turning to the left when they had been deprived of food for 22 hours, but turning to the right when they had been deprived of food for only three hours, 10 of the 12 rats had reached the criterion of learning, responding correctly on six successive critical trials.

In contrast to the difference in time of deprivation of 19 hours that was employed by Bloomberg and Webb (1949), Jenkins and Hanratty (1949) used a difference of 36 hours in length of deprivation, a difference that leads to greater discriminability but probably less significance.

One additional experiment serves to demonstrate the importance of the condition of deprivation during training. Cautela, (1956) trained rats to make a black-white discrimination under 23 hours of food deprivation and then extinguished the response under 0, 6, 12, 23, 47, and 71 hours of deprivation. The number of responses made during extinction increased across groups from 0 to 23 hours of deprivation, and then gradually diminished to the 71 hour deprivation group. The author interpreted these results as indicating that deprivation has both a cue and an energizing function. The data can also be considered to illustrate the general principle proposed by Skinner, that extinction will occur least rapidly when extinction and acquisition conditions are most similar.

These studies demonstrate that the stimulus correlates of varying conditions of deprivation can be discriminated by rats, and that these stimuli can serve as cues for differential responses. Phrased differently, the stimuli associated with conditions of deprivation have been demonstrated to exercise some measure of control over the behavior of the animals in the determination of the response that is made in a particular stimulus situation. These stimuli can serve as cues for various responses that need not be instrumental to the reduction of the need state that gives rise to the stimuli.

Motivational Function of Conditions of Deprivation

The cue function of conditions of deprivation, although it may be very important, has typically been eclipsed by investigations into the motivational aspect of deprivation. The typical experiment in learning utilizes animals that have been deprived of food for 22 or 23 hours for two very good reasons. First of all, this sort of schedule is not disruptive to the schedule of the experimenter. He can spend one or two hours in the laboratory each afternoon, following which the animals are fed and started on the next deprivation period. Secondly, following a prolonged period of deprivation, the animals respond very readily to food or water, whichever has been withheld. Deprivation, in this respect, reduces the variability of the behavior of the animal. The animals are disinclined to waste time sniffing about, preening, or dozing.

Deprivation as a motivational variable has been demonstrated innumerable times, occurring nearly every time that level of deprivation has been investigated as a parameter in behavior. Ramond (1953) presents rather typical results. He divided his rats into two groups and taught both groups a bar pressing response with food as the reinforcer. One group was deprived of food for 22 hours and the remaining group was deprived of food for four hours. Response rate was higher for the 22 hour deprivation group, but when he shifted half of each group to the other drive condition, the rats that were changed to the 22 hour condition immediately increased their response rate to a level commensurate with the rate demonstrated by those that had been initially performing under the condition of greater deprivation. The group that was changed to four-hour conditions also showed an appropriate decrease in their response rate. Cotton (1953) gave 20 albino rats trials in a straight alley for 44 days under random conditions of food deprivation, food having been removed 0, 6, 16, or 22 hours before the trials. He found that the mean running time decreased in roughly linear fashion with increasing deprivation. He had observed the rats for evidence of "competing" responses and when he performed an analysis of the running speeds with the trials involving competing responses eliminated, the effect was nearly washed out. Competing responses were defined as those involving retracing, face-washing, touching the cover of the runway, exploring the openings through which the photo-beam passed, scratching self, and biting self. This suggests again that the major function of deprivation is motivational in the sense that it produces behavior in the rat which the experimenter wants to occur and suppresses other behavior.

Perhaps a better example of the motivational effect of a condition of deprivation is afforded by Reynolds and Pavlik (1960). Rats that had been maintained on a 22 hour feeding schedule were split into three groups which were deprived of food for 3, 22, or 44 hours before being run to a .1, 1, or 2 grams of wet mash. In an analysis of variance, both main effects were significant at the .01 level of confidence when the analysis was performed using only trials run at asymptotic speed, the speed of the rats varying directly with the amount of reward and the time of deprivation.

Drive During Training and Habit Strength

Hull (1943), in his formulation of his learning paradign, relegated drive (which was defined as a function of deprivation) to the role of determining response strength, but he left it out in the determination of habit strength. Since that time there have been studies purporting to show that either it does or does not enter into the determination of habit strength. No matter what one's position in the matter, it is possible to find manifold support within the literature. Because of the abundance of criteria for learning, it is impossible to line up opposing findings, and any judgment on the basis of sheer numerosity would be fallacious because of the tendency for only positive results (in the sense of supporting a particular position) to find their way into print. Even statistical issues mitigate against the resolution of the problem, for in this instance most designs are such that the Hull hypothesis is precisely the one that the experimenter is testing. Given few learning trials and small samples, it is nearly impossible to show that there is a differential effect resulting from training under different levels of deprivation. Finally, there is the same issue that developed with respect to the latent learning problem, the question of whether or not performance reflects learning. If one is mindful that one has access only to performance and that learning is a construct, it will do no harm to examine some of the experimental findings that have been published in the area.

One of the earliest and most frequently cited experiments in the area of the relationship between drive level and learning was performed by Finan (1940). He trained his rats to press a lever to obtain food while 1-, 12-, 24-, or 48-hours hungry. After all had received an equal number of reinforcements, he recorded the number of responses during extinction with all animals under an equal drive level. The rats that had been trained under 1 hour of food deprivation emitted the fewest responses, the 12-hour group the most and the 24- and 48-hour group emitted somewhat less than the 12 hour group. The only differences of statistical significance were those between the one-hour group and the other three groups. Reynolds (1949) on the other hand, employed a different method of deprivation and obtained distinctly different results. His apparatus consisted essentially of a straight-alley maze. Instead of simply withholding food for a specified period, both experimental groups had received no food for 24 hours, but one group, the high drive group, had received only three grams of food whereas the low drive group had received 12 grams of food just prior to the beginning of the deprivation period. By normal standards, then, even his low drive group was a high drive group and one has to bear this in mind while interpreting the results. Both groups were given 25 reinforcements for the performance of an instrumental response and then all animals were permitted unlimited feeding for 24 hours. Following this, they were given 12 grams every 24 hours and on the fifth day, all animals were given extinction trials under equal levels of deprivation. The original low drive group required significantly more trials to extinguish than did the high drive group, presumably showing a differential effect in performance as a result of training under different levels of deprivation. It may be noted that these results are contrary to what would be expected, for it is typically argued that high drive should result in a greater habit strength and there are some data that tend to support this. But we have already noted two factors that could account for Reynold's results. The first, perhaps not of as great importance as the other, is the correlation between amount of activity and feeding regimen. The second is based upon the stimulus complex at the time of extinction testing. Both groups were run through an extinction series sometime after having been fed 12 grams of food, the amount that the "low drive" group had received prior to the training session. Since it has been fairly well demonstrated that the stimulus conditions present at the time of training can serve as a cue for the response, it would be expected on this basis that the low drive group would emit the greater number of responses during extinction.

Kendler (1945b) also used a measure obtained during extinction of an instrumental response. He gave two groups under different conditions of deprivation acquisition trials in a bar-pressing situation and then extinguished the response under a common level of deprivation. He found no significant difference in the number of responses emitted and concluded that the experimental treatment did not result in a difference in habit strength. In a further experiment he again employed two groups of rats under different conditions of deprivation, but this time in a T maze situation. All rats received acquisition trials in a T maze to a criterion of 12 successive correct responses. The low drive group required more trials to attain criterion, but required fewer trials in relearning the problem, presumably because of the greater number of reinforcements during the original acquisition trials.

Further support for this position is offered by Straussburger (1950), who used rats and a bar-pressing task. He used deprivation periods of 1/2, 1, 4, 11, 23 and 47 hours in conjunction with 1, 10, or 30 reinforcements. The 23-hour deprivation group required less time to acquire their fixed number of reinforcements, but there was generally no relationship between level of deprivation during training and the number of responses, the rate of response, or the time required during extinction. He did find, as might be expected, that resistance to extinction was a function of the number of reinforcements received during training.

It appears that the failure of the aforementioned investigators to obtain results might be a result of comparing the fairly gross measure of total number of responses throughout the extinction series, for groups differing in drive strength during acquisition. Two investigators have obtained positive results with refined measures, although the effect may be due to the fact that both measure running time rather than number of responses. The first of these was reported by Lewis and Cotton (1957). They ran albino rats in a straight alley while deprived of food for 1, 6, or 22 hours, and with 50 percent or 100 percent reinforcement. Extinction trials were run with factorial subdivision of the acquisition drive levels. Throughout acquisition and extinction they measured the running speed of the rats and found that the amount of deprivation during training had a significant effect on running speed during acquisition and during the first 12 extinction trials, but it had no effect on the amount of spontaneous recovery. Extinction drive level, of course, had an effect on running speed during extinction. The authors concluded that drive level during training does indeed affect habit strength, but that the effect is relatively weak. Most of the effect was due to the performance of the one hour deprivation group.

Barry (1958) performed an experiment that in many respects is very similar. He also used two levels of deprivation during training, 2 1/2, and 26 1/2 hours, and ran his rats in a straight alley. The high drive group performed significantly better than the low drive group during training (.001) in terms of running speed. He also factorially subdivided his groups so that half of each group was extinguished under the opposite level of deprivation, and found an acquisition drive level effect on the first few extinction trials. During the first six extinction trials, the rats that had been trained while 26 1/2 hours hungry ran 1.88 feet per second whereas those trained under 2 1/2 hours of deprivation averaged only 1.46 feet per second.

Deese and Carpenter (1951) also minimized the importance of drive level during training. They placed 30 pigmented rats on a 24 hour feeding schedule, the rats receiving food one hour daily. During training in a three foot straight alley, half the animals were given acquisition trials just prior to their feeding and the remainder were trained just after the feeding, thus serving as a low drive group. Using latency as their dependent variable, they gave all animals 24 reinforced trials and then switched the deprivation conditions for half of each group. On the 25th trial, the animals who were switched to the longer deprivation schedule performed as well or better than the animals that were trained under the high deprivation schedule, and those that were switched to the low deprivation condition showed a dramatic decrement in their performance, although they did not have as great a latency as the animals that remained on the low deprivation schedule. They concluded that drive reduction (since they regarded the increase in performance of the group trained immediately after feeding as demonstration of latent learning) or in our context, deprivation during training was more nearly related to performance than to learning. Although they recognized that the rats that had been switched to low deprivation conditions maintained an advantage over the rats trained under low drive (sig. .05), the effect is slight.

Learning and Performance

It has been frequently argued that conditions of deprivation during training may have some effect when 'vigor measures' which are a function of time are employed as the dependent variable; but that measures of accuracy, such as error during acquisition, remain relatively free of the effect of deprivation (or drive), especially if the learning task is fairly complex. Teel (1952), in an experiment very similar to the one reported by Kendler (1945b) gave four groups of rats 56 trials in a single T maze under 1, 7, 15, or 22 hours of food deprivation. Following the acquisition series, he subdivided the groups again into groups of the same condition of deprivation and observed the groups for any evidence in difference in resistance to extinction as a function of the training drive level. Since there was no significant difference, he concluded that drive was not a determinant of habit strength. Cotton (1956) reanalyzed Teel's data and confirmed that neither acquisition nor extinction drive level had a significant effect on extinction as measured by the number of trials required to meet a criterion of two or more errors in a block of four free choices. He also noted that there was no evidence of an interaction between acquisition and extinction drive level.

Hillman, Hunter and Kimble (1953) report an experiment that places greater emphasis on the typical distinction between 'learning and performance.' They employed a fairly complex task, a 10-unit multiple T maze and used groups that had been deprived of water for two and 22 hours. After the first ten trials, half of each group was changed to the other condition of deprivation. They did not find a significant difference in the number of errors for either of the groups that underwent a change in deprivation, but did observe a change in running speed. In every group, running speed was directly related to the prevailing condition of deprivation.

A highly similar experiment was performed by Coyer (1953). He also used water deprivation as one variable and included an additional variable of amount of water reinforcement in a factorial design. He performed an analysis on both errors and speed, the two measures presumably measuring acquisition of learning and performance. While the amount of reinforcement in this instance was not related to either acquisition or performance, the time of deprivation was related to running time but not to the number of errors.

It is seldom surprising to find evidence that relationships are more complex than were originally supposed, this seems to be a frequent occurrence within psychological research, and in this instance Reynolds, Marx, and Henderson (1952) question the simplicity of the typical interpretation. Utilizing a factorial design with two levels of deprivation and two levels of reward, they found an interaction between these conditions in the number of responses during extinction. With a large reward, the high-drive animals extinguished more rapidly, but the low drive animals extinguished more rapidly with a small reward. They proposed a new theoretical construct which is the product of the drive level and the amount of reward to account for the results.

Moving briefly from conditions of deprivation to external stimulation, Campbell and Kraeling (1953) have shown that both the initial shock intensity and the amount of shock reduction will exercise an effect on the performance of a rat in a straight alley. Given a constant amount of reduction (in voltage), the rats experiencing the lower initial level of shock ran faster than the rats experiencing a higher level of shock. They also observed that performance was a function of the amount of shock reduction, and that while complete removal of shock from differing initial levels did not affect final running speed, it did influence the rate of acquisition of the running response.

It is difficult to know how to interpret other occasional interactions that appear in the literature, for it is recognized that many will occur simply on the basis of chance during the course of experimentation, especially in light of the current emphasis upon factorial designs. Typical of many is the one reported by Arbit (1956). He, as have so many, used two levels of drive as one dimension of a factorial design, and as another employed the presence or absence of drugs noted for their ability to impair the functioning of the autonomic nervous system, tetraethylammonium and hexamethonium. High drive during learning interacted with the presence of the drug to produce more efficient relearning of the maze.

Drive Level in Discrimination Learning

Most of the literature suggests that there is no advantage accruing from the introduction of a high drive condition for discrimination learning. The results of a typical experiment are exemplyfied by the results obtained by Lachman (1961) in a brightness discrimination problem. He forced high and low thirst groups to 25, 50 or 75 percent correct response. High thirst in this instance resulted in faster running, but not in better discrimination as measured by the number of correct responses. But these results contrast with those obtained by Eisman, Asimow, and Maltzman (1956). Three groups of rats, deprived of food for 4, 22, and 46 hours, learned a black-white discrimination (white positive). After the initial learning, the groups were sub-divided into new deprivation groups of 4, 22, and 56 hours and were trained to black positive. The lack of difference in performance between the four and 22 hour deprivation groups led the experimenters to develop a two parameter definition of drive by which they justified combining the two lower drive groups for their analysis. They found that the high drive group required fewer trials, fewer reinforcements, made fewer errors, and required more trials (hence showed greater resistance to extinction) in reversal learning. But it doesn't appear that reversal training is the key to positive results in the area, for Buchwald and Yamaguchi (1955) conducted a similar experiment with dissimilar results. Using a correction procedure, they trained four groups of rats to run to one side of a T maze to a criterion of 10 out of 12 correct responses when 1.5 or 20.5 hours thirsty. Following training, one of the groups from each training level of deprivation had the conditions of deprivation changed to the other condition and all subjects went through reversal training. This time, the high drive groups learned the reversal task more rapidly. It should be pointed out that different conditions of deprivation were used in the two experiments, food in the former and water in the latter, and this may in part account for the differences in results. It may also be noted that in the former experiment rats suffering 22 hours of food deprivation were classified as low drive (and were not distinguished from the results obtained with animals that had been deprived of food for only four hours), while in the latter experiment, the periods of deprivation were 1.5 and 20.5 hours, these periods of deprivation constituting the low and high drive conditions.

Bahrick (1952, 1953) has reported some rather interesting results that reveal a differential deprivation effect in sensory preconditioning. In the first experiment, albino rats were given 400 paired presentations of sound and light under varying degrees of food and water deprivation. Following this they were trained in an avoidance apparatus which paired sound with electric shock. Finally the subjects were trained with the pairing of light and electric shock. He found that the animals that had received the original pairings of sound and light under conditions of high drive generally acquired the avoidance response to light significantly more rapidly than the animals that had received the initial training under conditions of low deprivation. The second experiment was similar in design and yeilded similar results. In this case rats were exposed to 360 paired presentations of a light and a buzzer while either sated or deprived of food for 14 hours. A control group was exposed to the light without the buzzer while 14 hours food deprived. Following the initial training, they were taught an avoidance response to the buzzer by pairing it with shock, and were trained to make an avoidance response to the light. The group that had been deprived of food during the initial training again required significantly fewer trials to reach criterion for conditioning the light alone. These results, similar to those found in studies of generalized drive, might be explained in terms of lowered sensitivity to external stimulation as a result of deprivation. The research supporting this position has been covered in an earlier section.

Additivity of Drive Conditions

Conditions of deprivation tend to be regarded as irrelevant when there is no obvious connection between the need state of the organism and the reinforcing substance. Hence if a rat has been deprived of water but is running to food, the thirst 'drive' is regarded as irrelevant--even though it may have fairly dramatic behaviorial consequences. Hull (1943) postulated that the incidence of an irrelevant (or alien) drive would combine with a relevant drive to increase the total motivation (D) according to the following equation: $D=(\bar{D}+\dot{D}) / (\dot{D}+100)$, where $\bar{D}$. the strength of the relevant drive, and $\dot{D}$, the strength of the irrelevant drive, can vary from 0 to 100. It is readily seen that D increases more rapidly with increments in $\bar{D}$ than with increments in $\dot{D}$, that D has an incremental effect only if $\bar{D}$ is less than the theoretical maximum, and that the contribution of an increment in $\dot{D}$ varies inversely with the strength of $\bar{D}$. These are, then, the predictions that can be derived from this much of Hull's formulations. There are, of course, limitations imposed, for relevant drive, although a function of the time of deprivation (in the case of drives based upon homeostatic processes) is in no sense simply a linear function of time. A rat deprived of food for a week, rather than being very strongly motivated, is in all likelihood dead.

The experimental literature bearing on this subject is manifold and, as might be expected, conflicting, although perhaps not beyond resolution. The positive effect of the addition of shock to some other relevant drive condition seems to be very well established. Miller (1945) trained rats to run to food in a T maze when deprived of food. Following this training, he sated the animals and shocked half immediately before a trial. As predicted by Hull's formulation, the shocked group ran more quickly (and with fewer errors) than the unshocked group. Amsel (1950) conducted a similar experiment, but reversed the roles of the drives. He found that irrelevant hunger (22 hours food deprivation) failed to show an appreciable effect when the rats were running to avoid two levels of shock, but that there was a significant increase in running speed due to deprivation when the rats were running in an apparatus in which they had been previously shocked, conditioned fear or anxiety presumably being the relevant drive in this situation.

Amsel and Maltzman (1950) present further confirmatory evidence, again using shock but pairing it with a relevant thirst. They afforded female rats an opportunity to drink for 10 minutes each day. After the amount consumed had reached an asyomtic level, they shocked the rats immediately before they were given access to water. The rats showed a significant increase in the amount of water ingested, or, to phrase it differently, showed a significant increase in their drinking behavior.

Webb (1949) trained 88 albine rats to push a door open to secure food, reinforcing the response with a food pellet for 100 trials while the rats were on a 22 hour food deprivation schedule. He then sated four groups and extinguished them under 0, 3, 12, and 22 hours of water deprivation, a fifth group being extinguished under 22 hours of food deprivation, the same as the training conditions. There were positive, approximately linear relationships between the strength of the irrelevant drive and the number of responses made, the time the door was held open, and the time required to extinguish. The effect of 22 hours water deprivation, in terms of the extinction measure, was about 17 percent of that of the relevant drive condition. Brandauer (1953) replicated Webb's study, matching the experimental groups on operant rate of response under thirst drive. After conditioning the lever response to 11 hours hunger drive, he extinguished the response under 0, 11, and 22 hours thirst drive. Again, a positive relationship was obtained between the strength of the irrelevant drive and resistance to extinction, the response evidently transferring to the irrelevant drive.

In contrast to the simple tasks mentioned above, Braun, Wedekind and Smudski (1957) investigated the effect of an irrelevant drive in the performance of a relatively complex task. They ran 62 male rats in a five unit multiple T maze that had been adapted for swimming. For the condition of relevant drive, they employed water temperatures of 15°C and 35°C, assuming that the rats would be more strongly motivated to escape the cold water. Half of the animals were given food and lab. (with the exception of the time actually in the maze) and the other half were deprived of food for twenty two hours prior to the time of the experimental trials; the condition of food deprivation constituted the irrelevant drive condition. The group with the irrelevant drive, although doing more poorly on the initial trials, excelled the control group in both speed and the number of correct trials in reaching criterion.

While the results presented to this point lend to a highly consistent interpretation, not all of the results reported on this particular problem are so compatible with the Hullian principle of drive additivity. While drive may have an energizing function, this is not the only function of drive. At least two further consequences of a drive condition would lead one to expect results that are quite different from those reported above. These are the sensitivity to the other stimuli available in the environment (and the possibility that other responses are conditioned to these stimuli), and the possible competition of responses that have been conditioned to the irrelevant drive stimulus itself. Both of these result in the distraction of the experimental animal from the experimental task, and for this reason are discussed under the heading of attention.

Deprivation and Attention

Evidence already presented indicates that one of the main reasons for the manipulation of the period of deprivation in the typical learning study is that the procedure encourages the rat to get down to business and to attend to the task at hand. The research of Campbell and Sheffield (1953) and Teghtsoonian and Campbell (1960) has demonstrated that much of the increase in activity that results from deprivation can be attributed to increased sensitivity to environmental stimulation. The results obtained by Cotton (1953) have also been mentioned, but will be briefly reviewed in the present context since they provide additional data on the problem. It may be recalled that Cotton ran rats in a straight alley maze after they had been deprived of food for 0, 6, 16, or 22 hours. Although he found a linear relationship between running speed and time of deprivation, when he eliminated the trials on which there had been evidence of competing responses, e.g. retracing, face-washing, scratching, etc., there was no difference in running speed for the various groups. In other words, the higher the drive level (or the longer the period of deprivation), the less likely was the rat to engage in other behaviors. Once again, it appears that the major function of deprivation is that of making the animal attend to the reinforcing stimulus.

Essentially the same sort of conclusion was drawn by Schlosberg and Pratt (1956), but their conclusion was based on entirely different grounds. They had run hungry and satiated animals in a T maze, one side of which was baited with inaccessible food. The trials were conducted in pairs, the rats responding freely on the first trial on each pair and being forced to the opposite side on the second trial. In terms of first trial performance, the satiated rats showed no evidence of learning, and when they were subsequently subjected to conditions of food-deprivation they required the normal length of time to develop a preference for the side of the maze that provided secondary reinforcement. On these and other grounds, the authors proposed an activation theory of drive, suggesting that the immediate effect of drive is that of attention, and the long term effect is that of learning.

It is difficult at the present time to draw out all of the ramifications of this data on attention, but there are additional data to suggest that the consequences of a consideration of attention will be far reaching. Among these are those provided by Carper (1953) and Smith and Kenney (1956). Carper was interested in the reinforcing efficacy of glucose and saccharin for rats experiencing specific and general hunger. Specific hunger was induced by introducing a caloric deficiency into the diet of half of the animals and general hunger was defined by the operation of withholding food for a period of 12 hours (experimentation was conducted before or after feeding on a 12 hour schedule). He combined the three variables factorially and tested the rats in a Skinner box on a periodic schedule of reinforcement that reinforced 67 percent of the responses. The surprising result is that he found that there was a significant interaction between the reinforcing substance (saccharin or glucose) and the period of deprivation (zero or 12 hours). The induction of a condition of hunger resulted in a decrease in the rate of response to saccharin. Also, saccharin was less reinforcing for rats with a specific dietary deficiency than it was for the rats enjoying an adequate diet. Glucose, on the other hand, was more reinforcing for the rats with a dietary deficiency, and was generally more reinforcing for rats experiencing general hunger. The results are all the more interesting in light of the fact that the solutions (.13 percent saccharin and 7.0 percent glucose) had been selected for their equal-reinforcing-value for satiated rats. It is quite apparent, whatever the reason, that the introduction of a condition of need results in the substances being discriminated in a manner in which they are not discriminated by satiated animals with the result that saccharin is less reinforcing for the animals with a condition of irrelevant need.

If the findings of Carper were isolated in the literature, it would be somewhat tempting to suppose that they reflect the hazards of statistical procedures and to subsequently dismiss them from further consideration. But a highly similar finding has been reported by Smith and Kinney (1956). Instead of using glucose and saccharin, they used sucrose and water, but again with satiated and food deprived animals. They also used a Skinner box and a partial schedule of reinforcement, one obtained by distributing 16 reinforcements over a 46 minute experimental period. The interaction between reinforcing substance and condition of deprivation was significant at the .05 level of confidence; while the satiated animals responded to sucrose and water with about equal frequency, hungry rats responded less frequently to water but more frequently to sucrose. Since food deprivation induces a condition of need that is irrelevant to either saccharin or water reinforcement, both of these studies provide an instance in which the introduction of an irrelevant drive depresses the rate of response to a reinforcing stimulus.

It cannot be said with certainty whether or not the data provided by Kendler (1945a) is the result of the same sort of mechanism, but his results were similar in many respects to those obtained by Carper (1953) and Smith and Kenney (1956). In an effort to test the Hullian postulate concerning drive additivity, he trained rats in a black-white discrimination habit in a T maze while hungry (deprived of food for 22 hours), and deprived of water for 0, 3, 6, 12, or 22 hours. Contrary to the theoretical expectation (and the results obtained by Webb), the drives summated to produce a greater number of responses to extinction in a Skinner box following 30 reinforcements up to the point of 12 hours water deprivation, but beyond that, the addition of irrelevant drive resulted in a decrement in performance.

The same effect was present in results reported by Siegel (1946). He also conditioned a lever pressing response for food under conditions of 22 hours food deprivation and extinguished the response in one group while the drive conditions prevailing during acquisition, and in an experimental group that the addition of 22 hours water deprivation. The former group, the group that did not have the addition of the irrelevant drive, showed greater resistance to extinction, emitting more responses during the extinction trials. Although the trend is the same as that reported by Kendler, the differences did not reach a significant level and Siegel interpreted the results as confirmatory of the Hullian hypothesis of drive additivity.

Kendler and Law (1950) have pursued the topic further. In their first experiment they ran one group of rats that had been deprived of food for 22 hours in a single T maze with food as a reinforcer. Another group of rats were in addition deprived of water for 22 hours, although there was no water offered in the goal box. The group with the addition of water deprivation made significantly more errors in reaching a fixed number of reinforcements, again the addition of an irrelevant drive results in a decrement in performance. In a second experiment, they used water deprived rats with water as a reinforcer and obtained essentially the same results. The addition of a need for food resulted in more errors in reaching a fixed number of reinforcements.

Their results suggested that the addition of an irrelevant food drive had a greater retarding effect than the introduction of an irrelevant need for water, but since the two experiments had been performed with two different strains of rats, no direct comparison could be made.

The experiment performed by Kendler and Law (1950) was replicated by Levine (1956) who conducted both experiments with rats of the same strain, thus permitting direct comparison of the effects of the two conditions of irrelevant deprivation. Of the rats running to food reinforcement, those that were simply deprived of food had an average of 7.04 errors in reaching a criterion of 90 reinforcements in comparison with 9.25 errors for those that had the addition of 22 hours of irrelevant thirst, a difference significant at the .001 level of confidence. The second experiment, with a relevant drive of 22 hours thirst (water being used as the reinforcer) and the addition of 22 hours hunger for the irrelevant need condition, yielded a mean of 9.62 errors for the relevant need condition and a mean of 13.43 errors with the addition of the irrelevant need condition. This difference is significant at the .02 level of confidence. Levine also observed that the addition of irrelevant hunger, when combined with a condition of relevant thirst, had a greater retarding effect than the addition of irrelevant thirst to a condition of relevant hunger, an effect suggested by the Kendler and Law data.

Drive and Task Interaction -- Yerks-Dodson Law.

The previously cited studies collectively suggest that the Hullian formulation of drive summation has serious limitations, all of them showing that at higher levels of drive, the addition of a strong irrelevant drive has a depressing effect on response strength. This effect is generally predicted by the Yerkes-Dodson Law. This law states that there is an optimal level of motivation for every task and that the optimal level of motivation varies inversely with the difficulty of the task. Thus for a very simple task, one would expect that performance would show an improvement with the increment in the combination of relevant and irrelevant tasks until this combination reached a very high level. But with a fairly complex task, an increment in drive level might be expected to depress the level of performance. There is presently no clear-cut quantification of the variables involved in this formulation, but several studies have been completed that provide confirmatory evidence. The earliest experiment was completed by Yerkes and Dodson (1908). They used mice in an apparatus that afforded the mice a choice between two doors that led into lighted compartments. The differences in illumination were varied such that the discrimination was relatively easy, intermediate, or relatively difficult. If the mouse made the correct choice it was permitted to return to its nesting cage, but a wrong choice resulted in one of several intensities of shock. An analysis of the number of trials required for the mice to reach criterion in the discrimination problems clearly demonstrated the aforementioned relationship. The number of trials required to reach criterion was generally a curvilinear, U-shaped function of the shock intensity, and the optimal level of shock in terms of learning efficiency varied inversely with the difficulty of the discrimination.

Cole (1911) conducted essentially the same sort of experiment with two-day old chicks. For an easy brightness discrimination, the number trials required varied as a direct function of the shock intensity, the same as in the Yerkes-Dodson experiment, but the intermediate shock proved to be the optimal level of shock for a problem of intermediate difficulty. In this case the results for the difficult problem were confounded by the fact that some of the chicks refused to make a choice after having received several fairly intense shocks.

Dodson (1915) performed another experiment with kittens which yielded results consistent with the general law named after him and Yerkes. He also used a brightness discrimination problem and three levels of shock intensity. Eighteen kittens were given 10 trials daily in an apparatus in which a response to the lighter of two compartments resulted in escape from the apparatus, but a response to the darker resulted in shock. The results for the easy discrimination problem was essentially the same as those obtained by Yerkes and Dodson (1908) and Cole (1911). The number of trials necessary to reach the criteria varied inversely with the strength of the shock. On the problem of moderate difficulty, the difference between the medium and strong shock was slight, presumably both shock intensities being about equidistant from the optimum for that particular discrimination problem. The kittens with the difficult discrimination problem required more trials with strong shock than did those with moderate shock, showing again that as the problem difficulty increases, the optimal level of shock decreases, greater efficiency in learning occurring with the lower shock intensities.

Differential performance as a function of shock intensity was demonstrated with human subjects in an experiment by Vaughn and Diserens (1930). They used stylus mazes that automatically gave the subject a shock whenever he entered a cul or blind alley. All thirty-two subjects received trials with three different shock intensities and no shock being associated with the four stylus mazes. The analysis, performed on the time spent in the maze, showed great variance within blocks, but the mean trends were quite clear. The light shock trials were performed more quickly than the no shock trials, but the trials with the greater shock intensities resulted in more time being taken to complete the problem. With strong shock, which resulted in the slowest performance, the subjects became more hesitating in their performance. Often they would make more errors, but the time spent in the blind alley was much shorter with the strong shock than with the weak shock.

Broadhurst (1957) has provided further evidence of the validity of the Yerkes-Dodson generalization in a study using rats as subjects. The task involved discriminative learning in an underwater Y maze. The rat was required to discriminate between the brighter of two surfaces. The difficulty of the task could be varied by adjusting the difference between the lights. Motivation was varied by producing oxygen deprivation by underwater immersion. In the first part of the study, Broadhurst demonstrated that there was an optimum level of motivation for speed measures of performance. In addition, Broadhurst measured the emotionality of each rat by placing it in a brightly lit open field and counting the number of fecal boluses produced in a given period of time. He hypothesized that the more emotional rats should have a lower optimum level of motivation than the less emotional rats. When the rats were scored in terms of the number of errorless runs in 100 trials, a significant interaction effect was found between level of motivation and task difficulty as would be expected on the basis of the Yerkes-Dodson generaliztion. However, the same interaction was not found for speed of traversing the maze. The difficulty level appeared as a significant main effect in terms of the analysis of the number of correct trials.

In an additional experiment based upon the Yerkes-Dodson law, Dennenberg and Karas (1960) used an apparatus designed after that of Broadhurst (1957). Preliminary results from a straight alley under water test showed that both time of deprivation (underwater detention) and days (five trials per day) resulted in faster swimming time, corroborating Broadhurst's findings, but the data from the Y maze were in only partial accord. They had given the rats five trials daily for ten days and found a days by motivation interaction in their analysis of variance for time scores. An examination of their data revealed that during the initial trials, the introduction of an extraneous drive depressed the level of performance, but that the effect vanished as learning trials progressed. Since the difficulty of the task is a function of the number of trials, fewer errors being made in the later trials than in the early trials, the interaction can be regarded as a motivation by difficulty interaction, the effect expected on the basis of the Yerkes-Dodson law.

Braun, Wedekind and Smudski (1957), in an experiment mentioned above, ran 62 male rats in a five unit multiple T maze which was adapted for swimming. Relevant drive conditions were manipulated by using two water temperatures, 15°C. for high drive and 35°C. for low drive. The irrelevant condition of motivation was produced by depriving half the subjects of food for 22 hours. Performing their analysis on both log time and errors, they found that both low water temperature and food deprivation resulted in increased performance as measured by both swimming time and the number of errors made in the maze. Of more importance in the present context, they also found a triple interaction between time of deprivation, water temperature, and trials. An examination of their performance curves shows that during the early trials, the combined drive group did not perform as well as the single drive groups, but that they showed superior performance during the later trials. Dennenburg noted essentially the same effect in his experiment and attributed the effect to a decline in problem difficulty as a function of accruing experience. Certainly it can be argued very effectively in the present situation that the trials dimension in the analysis constitutes a dimension of diminishing levels of difficulty. The mean number of errors declined from 11 for trial one to about three on trial six. Using the number of errors as a measure of the difficulty, then, there is a decline in the difficulty and the interaction can be interpreted as one between difficulty and the combination of drive. The depressed performance of the combined drive group during the early or difficult trials, in this instance further corroborates the Yerkes-Dodson law.

Hammes (1956) has also demonstrated an interaction between task difficulty and drive level, in this instance fear conditioned to the stimuli associated with shock. He provided a box with an electrified grid and two doors, one of which provided the rat with escape from the shock. The electrification of the grid was provided by a 10,000 v. rectified current at three shock intensities, 20 ma, 30 ma, and 40 ma. The discrimination problem was set by equipping the doors with three-inch square cards, one black the other white for the easy discrimination, and a three-inch square white card as opposed to a one-and-a half inch white card for the difficult discrimination. Using the number of errors committed in attaining a criterion of 10 successive correct trials, he found that the high drive group (experiencing 40 ma current) did better, although not at a significant level, than the two lower drive groups. On the difficult task the two low drive groups did not differ significantly but both performed significantly better than the high drive group.

Interaction of "Anxiety" and Task Difficulty

It has long been a matter of interest to clinical psychologists that mental disorders have shown an effect on rate of conditioning. But a review of a few scattered studies by Hilgard and Marquis (1940) showed no consistent relationship between ease of conditioning and diagnostic category. Welsh and Kubis (1947a, 1947b), in an effort to reconcile the conflicting results of previous studies, attempted to minimize the reactivity of the subjects to the conditioned stimulus by using nonsense syllables, and used the shock and the PGR as the unconditioned stimulus and response. Using the number of trials to criterion as the measure of conditioning, they found that in-patients at the Payne Whitney

Psychiatric Clinic were readily discriminated from normal subjects taken from the student population at Hunter College. The students required a mean of 21.86 trials as contrasted to a mean of 7.54 trials for the patients to reach the criterion. Further, there was hardly any overlap in the distributions of scores for the two groups, a score of 14 trials discriminating the two groups with 98 percent efficiency. Within the hospitalized group, the performance of the subjects corresponded quite highly with staff evaluations of the extent of anxiety within the subject, further confirming that conditionability is related to anxiety. Since rate of conditioning had been empirically related to extent of anxiety of the patients, Welsh and Kubis used the conditioning score as a measure of anxiety and did further analysis of the reliability coefficient of .88. The correlation representing reliability for the patients was lower, but the changes in score were reputedly associated with changes in the amount of anxiety manifested by the patient on the occasion of the second test.

The technique was used with children in a later study reported by Schiff, Dougan, and Welsh (1949). Children hospitalized at the Bellvue hospital required an average of 18.1 trials as contrasted with 22.5 for children at the elementary school at Hunter College and 34.7 for children in public school 177 in Manhatten. The inordinately high mean at the public school is related to the fact that 26 of the 36 subjects failed to condition throughout the course of 40 trials. These data suggest that the phenomenon observed within the adult populations holds also for children, that is, conditionability is a function of anxiety.

Some years later, in an attempt to obtain a measure of irrelevant drive in order to test the Hullian postulate which expresses excitatory potential as a multiplicative function of drive and habit strength, Taylor had clinical psychologists select the items from the MMPI that they thought were indicative of anxiety. The resulting scale (Taylor, 1953) was then used to select subjects for experimentation. Taylor (1951) selected subjects with extreme scores and conditioned the eye-lid response. Quite in accord with the results previously reported by Welsh et al., the high anxiety group conditioned more readily than did the low anxiety group. She interpreted these results as confirming the Hullian postulate as well as supportive of her contention that the test was an adequate measure of anxiety.

Montague (1953) had previously examined the relationship between anxiety and rote serial learning. He used three lists of nonsense syllables that varied in difficulty as a function of intralist similarities and association values and found that anxious subjects performed better than the non-anxious subjects on the easy list, but performed at a lower level on the difficult list. The two groups were not differentiated on the list of intermediate difficulty. Taylor and Spence interpreted these results in terms of Hullian theory. They reasoned that with the easy list, the dominant response would be the correct response, hence the multiplication of habit by drive would result in improved performance for the high anxiety group. In the case of the difficult list, the dominant response was not necessarily the correct response and competing responses were consequently multiplied by drive to result in a relative decrement in the performance of the anxious group.

In order to test their reasoning, Taylor and Spence (1952) devised a "verbal maze" that was presented on a memory drum. The task employed two words, "left" and "right" each of which was presented 10 times in a quasi-random order on the memory drum.

Using extreme cases based on the results of the Manifest Anxiety Scale, they recorded both the number of errors and the number of trials required to reach the criterion of two successive errorless trials. The high anxious group required more trials and made significantly more errors in reaching criterion than did the low anxiety group. They also report that they observed a definite tendency for the high anxiety group to make relatively more errors on the more difficult choice points and relatively fewer errors on the easier choice points, a finding that further corroborates their hypothesis.

Farber and Spence (1953a) confirmed the general findings reported above in two experiments. In the first experiment, they employed a ten unit multiple T stylus maze for which they had data concerning the relative difficulty of the various choice points. Using undergraduate psychology students that were selected from the upper and lower ends of a distribution of Manifest Anxiety Scale scores, they recorded both the number of errors and the number of trials required to reach a criterion of two successive correct trials. On both measures, the low anxiety group excelled the high anxiety group in their performance. As in the previously reported study, the differences between the two groups was greatest in the number of correct responses on the relatively hard choice points and was at a minimum on the easier choice points. In the second study, they replicated the previously reported experiment with essentially the same results. The anxious subjects, in eye-lid conditioning, were superior to the non-anxious subjects. On the basis of these results it appears that the effect of drive (as measured by the anxiety scale) depends upon the experimental task. Even if one should reject the scale as a measure of drive, the findings reveal individual differences that result in differential performance in a learning task that must be taken into account.

Spence and Farber (1953, 1954) followed these results with two additional experiments that confirmed their earlier findings concerning ease of eyelid conditioning. Again using subjects that were selected for their extreme scores on the manifest anxiety scale, they first increased the interval between the conditioned stimulus and the unconditioned stimulus to one that is not conducive to ease in conditioning. Their results indicated that there was a significant difference (.05) both during conditioning and extinction in favor of the anxious subjects. Further analysis of their data showed that women performed at a higher level than men. In the second study, they investigated differential eye-lid conditioning. The subject was presented with a discriminable difference in the conditioned stimulus, one level being reinforced and the other never being followed with the unconditioned stimulus, an air puff. Using the differences in response to the reinforced and unreinforced conditioned stimulus as the dependent variable, they found again that the high anxious group performed better than the low anxious group.

Korchin and Levine (1957) as well as Spence, Farber and McFann (1956) and Spence, Taylor and Kretchel (1956) have generally confirmed the previous findings relating to the interaction between drive level (manifest anxiety) and the difficulty of the task when expressed in terms of competition of responses. The same effect was confirmed in a further study by Lee (1961). In the latter study, Lee gave preliminary training in a paired associates learning task, and then changed some of the pairs, left other pairs unchanged, and introduced some new pairs for further learning. As predicted by the Spence hypothesis, the high anxiety subjects did well on the unchanged pairs, but these people were inferior on the changed pairs. An interesting fact relating to this study is that Lee also used shock to induce a drive condition and found that this also resulted in an interaction with task difficulty. Further, since the triple interaction was not significant, the author concluded that the effect of manifest anxiety is chronic.

The major effect noted to this point, the interaction of anxiety and difficulty level of the problem was first investigated by Lucas (1952) in serial learning. He noted that the difficulty of a serial list was a function of the duplication of consonants, and found an interaction between anxiety and duplication in the analysis of his results. He also introduced stress, as an experimental variable induced by reports of failure, and found a positive relationship between the number of reports of failure and the advantage enjoyed by the non-anxious group. The importance of this variable was confirmed in the research performed by Montague (1953, cited above) and was further demonstrated in two additional experiments by Deese et al. In the first of these, Deese, Lazarus, and Keenan (1953) employed a measure of neuroticism that had been previously adapted from the Minnesota Multiphasic Personality Inventory by Winne (1951). The test is of particular relevance here for it has many items in common with the MAS and appears to be a measure of the same characteristic. Using subjects that were selected from the upper and lower quartiles of the test, they presented a list for rote serial learning that had a minimum amount of duplication within the list. In addition, they ran the subjects under one of three conditions, neutral, avoidance, and nonavoidance. The neutral conditions were fairly standard conditions for serial learning, the avoidance conditions paired a shock with responses that were not correlated with correctness. The subjects that were in the high neuroticism group had a slight advantage over the low group under the neutral conditions and increased the advantage under the other two conditions. The low group showed considerable impairment of performance when the shock was associated with wrong responses, while the high group showed a slight increase in performance; and when the shock was uncorrelated, hence unavoidable, the low group again showed a marked decrement in performance that was not shared by the high group. A second analysis of the data on the basis of MAS scores yielded the same relationships. Lazarus, Deese, and Hamilton (1954) varied the amount of intralist duplication. The procedure was identical with the one reported in the previous study with the exception of the use of the MAS rather than the neuroticism scale and a list of 12 consonant syllables with only five different consonants. Using an analysis of variance design, they found that none of the results were significant. They then proceeded first to compare performance in this study with the one previously cited and showed that the first list, with little duplication, was far easier than the second. They pointed out that all of the differences, although not significant, were in the expected direction. Specifically, they found that with a difficult task the anxiety group did more poorly and the avoidance interaction noted in the previous study is reversed, the groups doing better under these conditions than they did under the neutral conditions.

While the results on the relationship of anxiety to learning have been developed almost exclusively with the use of undergraduate students, some data have been presented to show that the same sort of relationship will obtain at lower grade levels.

Castaneda, McCandless, and Palmero (1956) administered a modified form of the MAS to fourth grade students and formed groups of high and low scorers. They then gave the children repeated trials with an apparatus that contained four differently colored lights that were paired with two buttons, each of which would turn off two of the lights. An additional light would indicate success. While both groups showed an improvement in performance across trials, the anxious group was consistently and significantly lower than than the nonanxious group.

In an additional study employing a measure of anxiety, Castenada, Palmero, and McCandless (1956) used the same apparatus that was briefly described above but in this instance performed their analysis on the basis of the difficulty of the various light button combinations. They found through an analysis of variance, that the level of anxiety was not a significant main effect (although it would be expected to be on the basis of the total results cited above) but that anxiety interacted significantly with difficulty. An examination of the means showed that the high anxiety group excelled the low anxiety group on the easy items, but did slightly more poorly (but not significantly) on the more difficult items.

In addition to the exploration of anxiety as a condition of drive, several studies have been reported that investigated the effects of task stress and tension on learning. Castaneda, and Palmero (1955) designed a study that was based upon the assumption that stress, produced by the emphasis of speed in task completion, would affect the level of drive. Their task was similar to the one previously reported, - a matter of pairing the appropriate response button to a light stimulus. To test the hypothesis that high drive would facilitate performance when the correct response was dominant and impair performance when the correct response was not dominant, they assigned 108 fifth graders to one of four experimental conditions. In conditions I and II, the subjects were given 25 preliminary training trials, while subjects assigned to conditions III and IV were given 50 preliminary training trials. As another dimension in a factorial design, groups I and III relearned under nonstress conditions, while groups II and IV experienced stress as a result of an emphasis being placed upon the speed of performance. After the preliminary learning, some of the light-button pairs were changed while others remained the same. In the case of the changed pair it would be expected that an incorrect response would be initially dominant, whereas in the unchanged pairs, the correct response would predominate. In line with the findings from Spence's laboratory it would be predicted that the stress group would do better on the unchanged pairs, and that the non-stress group would do better on the changed pairs. It might be expected that the major effect of training would be to maximize these expected differences and enter into a triple interaction with change and stress. This effect was found to be significant, as was the effect of stress and the change in light-button relationships, both of which resulted in an increase in the number of errors. Further analysis showed that all of the F's computed on the basis of the unchanged scores were less than unity, indicating that any differences within these scores were probably not reproducable. A separate analysis on the changed S-R connections produced an interaction between training and stress that was significant at the .001 level of confidence. The fewest number of errors was committed by the nonstress group that had the advantage of 50 preliminary trials, relatively more errors being committed by the group that had had fewer trials. The introduction of stress had a slight decremental effect at low levels of training, but showed a dramatic decremental effect on the subjects that had strong S-R connections formed in the preliminary training.

In a second study, Castaneda (1956), using essentially the same apparatus as before, gave 32 trials (eight on each of the four lights) and then investigated the effect of stress induced by task pacing on the performance of a new learning task that consisted of two of the same associations and two associations that had been changed. Stress again tended to facilitate performance in the unchanged pairs and to have a deleterious effect on the performance of the subject for the changed pairs.

Clark (1962) also conducted an experiment that used stress induced by pacing (permitting a short response period) as an experimental variable. The apparatus consisted of means for presenting a horizontal light bar which was dissected at one point with a short vertical bar. The subject was provided with a movable response bar that was to be placed in the same relative position in the response apparatus as the vertical bar was in the stimulus apparatus. All subjects were given 21 trials with the vertical stimulus remaining in the same location. During these trials, a record of the response location was maintained for use in the second phase of the experiment. Following the initial trials, either the dominant response (the most frequent on previous trials) or a nondominant response which occurred one semi-interquartile away from the dominant response was reinforced with an audible signal. For the easy task, the stress condition produced the best performance for the first ten trials, but following this both groups did about the same and showed considerable variability and fluctuation. The groups were not differentiated with respect to performance on the difficult task in which the nondominant response was reinforced.

Osler, (1954) using long division problems with eleventh grade students, gave one form of the test in the first experimental session and then just prior to the second session in which they were administered a comparable form of the task, introduced experimental conditions designed to vary drive. One group received an officious note indicating their failure on the earlier test (all tests were given in the context of the school). The students in a second group received notes indicating their failure and also a note to each student telling him to report to the office since there had been a serious complaint concerning his conduct. Still another group was informed of its success on the first test but was told to report to the office. A fourth group was simply told of their success on the previous test and finally a control group was informed that they were being tested on an experimental basis and that no record would be maintained of individual performance. The final ranking of the means adjusted for pretest results are about what one might expect on the basis of the studies previously cited. The failure-fear group was characterized by the poorest performance, followed, in order of increasing performance, the failure group, the success-fear group, the control group, and finally the success-control group.

Other conditions of stress used in recent literature involve reports of failure and conditions typically assumed to involve a high degree of stress, e.g. doctoral exams. Beam (1955) took advantage of the actual stress situations that normally occur on a university campus. He had 54 subjects learn a list of nonsense syllables prior to their experiencing a 'real-life' anxiety arousing situations, such as doctoral exams, giving oral reports, or appearing in dramatic productions. An equivalent list was learned under neutral conditions. Under both conditions, the experimenter recorded a palmar sweat index and conditioned the galvanic skin response to light with shock serving as the unconditioned stimulus. He also obtained a measure of manifest anxiety on each subject. His results indicate that stress facilitated conditioning, but interfered with serial learning . Further, the decrement in serial learning was related to the magnitude of palmar sweat index, but was in this instance unrelated to the Manifest Anxiety Scale. This latter device was unrelated to any of the other measures.

Johnston (1955) used an electric maze and two types of instructions, orienting or high motivating instructions and low motivating instructions. Under high motivating instructions both groups, consisting of subjects scoring high or low on achievement imagery, performed at about the same level, but the group high in achievement imagery did better than the other group when working under the low motivating instructions. Similar results have been reported by Karolchuch and Worell (1956) and Hurley (1957). Both investigators found that there was an interaction between the instructions given in a task and the amount of achievement imagery reflected in a paper and pencil inventory. Both found that under low motivating instructions there was a differential favor of the high achievement groups but that the difference either diminished or vanished with the introduction of "motivating" instructions. On the basis of the results cited above, it would appear that there is some quantifiable characteristic that is conducive to attendance at a task, but this effect seems quite different from that noted for anxiety. It seems rather important to note that in none of the achievement motivation studies was there a decrement in performance due to the introduction of the motivating condition. But it does seem that some subjects, when placed in a situation that affords an opportunity to complete a task, respond to the achievement challenges of the situation whereas others fail to react to the stimuli.

The foregoing studies add up to a rather impressive program of research and it seems unfortunate that there has been so many studies yielding conflicting results. In the case of the effect of anxiety in learning a stylus maze, Matarazzo et al. (1955) found a curvilinear relationship between Taylor Manifest Anxiety scores and the number of trials required to learn the maze. In this case, seminary students with scores in the intermediate range performed better than either the high or low scoring students. Their procedure differed from that of Farber and Spence in that the students were blindfolded during the trial, but it seems unlikely that this in itself would account for the discrepancy in results.

Hughes, Sprague, and Bendig (1954) attempted to replicate and extend the study of Taylor and Spence (1952) and used the same arrangement of "right" and "wrong" for a total of 10 presentations of each on a memory drum. In addition, since they thought that the difficulty of the list would be a function of the number of times that the subject had to alternate from one term to the other, they constructed lists that required

10 and 7 alternations in comparison with the 12 alternations required on the original list. Each list was presented to a high and low scoring group on a shortened form of the Manifest Anxiety Scale (Bendig, 1956) with a two second exposure of each stimulus word interval and a pause of two seconds between exposures. An analysis of variance performed on the results failed to involve the measure of anxiety in either main effects or interactions. In fact, the only result that attained statistical significance was the difficulty of the three lists. This resulted in a significant main effect. The authors noted that while there is a lack of congruance in the results, the lack of congruance extends also into the procedure employed in the course of the experiment. They had used a two second interval between stimulus presentations, a procedure evidently not followed by Taylor and Spence (1953). The importance of this rests in the fact that the introduction of a pause between stimulus exposures decreases the rate of acquisition; the tasks, although in one case the same, were not of comparable difficulty in the two experiments since they were learned with appreciably greater ease in the experiment reported by Hughes et al. But it hardly seems likely that this in itself would accound for the disparity with the Taylor and Spence results, for the main effect with the tasks demonstrated that the three lists did differ in difficulty, and at any point upon the difficulty continuum it seems reasonable to expect differentiation among high and low anxious subjects if indeed drive does combine multiplicatively with habit strength. This, of course, is based upon the assumption that the "difficulty" of a task is related to the extent to which it invokes competing responses.

Axelrod, Cowen and Heilizer (1956) in a widely cited study attempted to replicate the maze learning experiment cited above that was completed by Farber and Spence (1953) but found no significant difference in the performance of the anxious and nonanxious groups. However, they did find a significant difference in performance as a function of the lie scale score, the subjects with high lie scores making significantly more errors than the low lie scores. Although they reported that there was no significant relationship between anxiety level and item difficulty, the present reviewer took the liberty of reanalyzing their data and found a rho correlation of .81 between the rank order of the difficulty of each choice point and the rank of the difference in the number of errors for the high and low anxiety groups at each choice point. This correlation is significant at the .01 level of confidence, indicating that the superiority of the low anxiety group varied directly as a function of the difficulty of the task at the choice point. Further, assuming that each of the ten choice points on the task provided independent measures of the ability of the two anxiety groups, a sign test was employed to test the significance of the difference in performance. The high anxiety group had more errors in nine of the ten choice points, a difference significant at the .05 level of confidence. On this basis one would tend to reject the null hypothesis and conclude that the two groups do in fact differ in their ability on the task, the high anxiety group performing more poorly than the low anxiety group. A final finding in their study that warrants mention if not analysis is the interaction of the different experimenters and sex, and a triple interaction with anxiety, different experimenters and sex. The latter two variables, although of apparent importance, are often neglected in studies in this area.

Other conflicting results have not been so easily explained away. Among these (and there are far too many to permit complete enumeration) are those by L'Abate (1959), Saltz and Hoehn (1957), and Harleston and Cunningham (1961). Each of these failed to confirm the finding that high anxiety interacts with task difficulty (when defined in terms of intra-list competition or association values). Further, Bendig and Vaughan (1957) and King, Kimble, Gorman and King (1961) failed to confirm hypotheses related to discrimination learning and eyelid conditioning. It is certainly true that the literature fails to present one with a coherent body of consistent results.

When a reviewer comes across a series of inconsistent studies he is in a quandary concerning the conclusions that should be drawn. Certain facts over and above those presented here must be taken into account in drawing appropriate conclusions. One of these is that the major program of studies producing consistent results has been undertaken in the Spence laboratory at Iowa and in the laboratory of his student Janet Taylor at Northwestern University. Scientific history presents many examples of one group of investigators being able to produce consistent research results which are not, at least for a time, reproduced elsewhere. The common reason for this is that the particular group has developed a precise and meticulous procedure which others have not been able to duplicate. This may well be true in the present instance. Spence and his associates are well known for the meticulous care with which they undertake research, and the fact that they have produced a flow of consistent results is impressive evidence that they are dealing with a genuine phenomenon and not an ephemeral will-o-the-wisp. The present reviewers are inclined to take this position and place less emphasis on the near replications of the Taylor and Spence experiments which have produced negative results.

The plausibility of this interpretation is somewhat supported by the fact that it has been demonstrated that there are several other variables that will interact with anxiety, such as the sex of both the experimenter and subject to produce contrary results. It has also been suggested that manifest anxiety scores will vary as a function of the conditions of testing, and that the test is a measure of the susceptibility of the subject to the stress that may be characteristic of the experimental situation. Certainly it would be premature to attempt to list all of the variables that have to be controlled in order to obtain the desired level of reproducibility in experimentation, but it should be realized that there are sufficient contingencies involved that failure to replicate previous findings with our present state of knowledge should come as no great surprise. In line with this general interpretation, Prokasy and Whaley (1962) recently made some headway in explaining the conflicting results that have accrued with respect to the relationship of anxiety to classical conditioning. They noted that one of the variables that had not been previously controlled systematically was the use of a ready signal, and found that the use of a ready signal produced different results.

In addition to the foregoing considerations, experimental results that have accrued from outside of the area of learning theory provide good reason for assuming that the bulk of the findings demonstrate that we are dealing with a genuine phenomenon. Gynther (1957), for example, found that anxiety as measured by Welsh A scale (1956) related to efficiency in communication, anxiety having a detrimental effect on efficiency. Of further significance is the finding of an interaction between anxiety and classroom structure in school achievement. Grimes and Allinsmith (1961) assessed the amount of over-and-under achievement in reading for third grade pupils from two different school districts, one that employed a strong phonetic approach and one that relied strongly on a sight or "see-and-say" method of reading instruction. They found that the district employing the traditional approach generally produced over-achievement, and that the amount of over-achievement varied directly with the amount of manifest anxiety. The other district, on the other hand, was lower in achievement, and the amount of the difference between the two school districts varied directly with the amount of anxiety. The interaction between anxiety level and school district was significant at the .01 level of confidence with the greatest impairment resulting in the case of the high anxious students in the district employing the sight method. Grimes and Allinsmith interpret these results in terms of the degree of structuring that they found evident in the employment of the two teaching methods. They thought that the lessonswithin the district employing the phonetic approach were highly structured, the individual assignments being very well defined in contrast to the lessons in the other district. This interpretation is consistent with the results obtained by Noll (1955).

Breadth of Learning and Cue Utilization

Recent years have seen far greater effort concentrated on the problem of the interaction of motivation with experimental conditions, in part because of the increasing complexity of experimental designs and in part because of the greater experimental effort that has been expended in an effort to uncover the variables that have been responsible for the lack of reproducibility of previous findings. Concomitant with the increase in experimental effort, there has been an increase in the number of scientific articles relating the variables on a theoretical level, many of which have succeeded in integrating data that has accumulated in widely divergent areas. Several of these articles have, to a large extent, been stimulated simultaneously by the physiological studies that have explicated the ascending reticular activating system, more specifically in many cases, by Lindsley's theory of activation (1951, 1958), and the learning studies dealing with the Yerkes-Dodson law, Spence's theory of motivation, and the manifold studies that have related various and sundry measures of stress, tension, goal failure and drive to various measures of learning and performance such as those reviewed in the present chapter. Not to be neglected in this account is the impetus that has been contributed by the multitude of latent-learning studies that extend back to the original studies in the California laboratory (Blodgett, 1929) as well as the studies in incidental learning that have been popularized by Bahrick (1954, 1957) and Postman and his collaborators (1946, 1954, 1956, 1957).

The general view that is emerging is in one sense multifactored since it involves several components, but at the same time unitary since it also involves a coherent and integrated conception of behavioral dynamics. The first component is one based on a dynamic conception of the nervous system, much the same as has been argued by Hebb (1955), Leuba (1955), and Malmo (1959). These authors present a picture of the normal organism as one that is active, striving, and interacting with the environment rather than one that is occasionally goaded into action by some condition of need (as defined in the traditional manner) or by shock or some other aversive stimulus. In short, the animal is naturally motivated to engage in commerce with its environment. This type of conception is interjected most frequently to account for curiosity, exploratory, or manipulatory drives, or, as White has recently argued (1959) the need for competence. It is also presumed by many writers that this type of motivation is responsible for what has been termed latent or incidental learning, the particular term used depending upon the experimental design and the objectives of the experimenter. The important consideration at this point is the finding that the amount of incidental learning varies inversely with the strength of irrelevant need conditions. Bahrick (1954), for instance, found that a high incentive retarded the learning of incidental colors paired with geometric forms that were to be learned during the course of the experiment. He later found (Bahrick, 1957), that incidental learning occurred mainly during early learning trials, presumably while the subjects were uncertain of the task-relevant variables, and during the over-learning trials when the subjects were less motivated in relation to the experimental task.

Johnson (1953) systematically varied the strength of motivation in a latent learning situation and also found that the amount of latent learning varied inversely with the strength of motivation. In a review of many of the earlier studies in the area, Thistlethwaite (1951) also concluded that "the stronger the drive, the poorer the irrelevant incentive learning," as had Tolman (1948) at an earlier time. In a highly imaginative study, Bruner, Matter, and Papanek (1955) investigated Tolman's position and obtained results that were very broadly confirming. Their strategy was to run rats to food in an alley that was segmented with four pairs of doors, one of each pair being black and the other white. Two conditions of drive were used, one based on 12 hours food deprivation and the other based on 36 hours food deprivation. The members of each of these groups were further subdivided into groups receiving 30 and 100 trials in initial training to run to either black or white doors (the color-positive door being the only one that would permit entry into successive sections of the alley). Following initial training, the pattern of the positive doors were placed in a single alternation sequence for an additional 20 trials after which all doors were changed to grey and the animals were required to learn a single alternation sequence in order to traverse the alley efficiently for the purpose of obtaining food. Neither the animals that over-learned (100 preliminary trials) nor those experiencing high drive conditions realized a significant savings in the number of trials required to learning the alternation sequence, but a significant saving was shown for the complementary groups, those with the lower drive conditions and those that only learned to criterion (30 trials in the pre-training).

While there have been many studies such as those mentioned above that demonstrate a deleterious effect resulting from increased drive conditions, the exact mechanism underlying this has as yet to be fully explicated. Spence's drive theory (1956), while permitting post hoc explanations is probably not sufficiently vigorous since it has been noted (e.g. Hill, 1957) that alternative deductions can be derived from the theory. The most promising effort that has been made to specify the conditions under which high drive conditions will inhibit or retard the rate of learning is one based on a concept of cue utilization. Easterbrook (1959) made an extensive review of the literature and on the basis of his interpretation of the experimental results, suggested that increases in drive result in a narrowing of attention much in the same manner as was suggested by Schachtel (1954). At low levels of drive, the animal is broadly aware of his environment and is aware of a wide range of stimulus change. But as a condition of need is introduced, there is a sharpening of attention on a narrowing range of stimuli as is suggested by the notion of hypothesis-testing in the discontinuity theories of learning. The data becomes more meaningful when the assumption is made that there is an optimal range of cue utilization for each task. Some data that has accumulated demonstrates a U or an inverted U shaped distribution when a learning measure is plotted against drive level. In a simple conditioning experiment, the range of relevant stimuli is greatly restricted and high drive may be thought of as a facilitory factor. In more complex tasks, however, the range of relevant stimuli increases and high drive is detrimental. The animal is unable, in the extreme, to span the entire range of stimuli necessary for mastery.

Although it is impossible to review all of the pertinent findings in this area (the interested reader is referred to Easterbrook, 1959), there has been one study that very nicely demonstrates the predictive power of the formulation in terms of breadth of attention. Agnew and Agnew (1963) found that high drive conditions generally impaired performance on Porteus mazes, but that such conditions generally facilitated performance on a task that involved reading color names while disregarding the irrelevant colors in which the names were printed. This latter finding is in opposition to that which would be predicted on the basis of Spence's theory (1956).

A further study that confirms the general theory is provided by

Quartermain and Scott (1960). The subjects were instructed to find a key that was hidden among various objects. The objects themselves were classified into three groups, those that could obscure the key, those that possibly could, and those that could not. When the subjects were later tested for the extent of knowledge that had been gained concerning the objects themselves, it was found that the degree of incidental learning varied directly with the degree of goal relevance. More of the items that could obstruct goal attainment had been learned than items that were irrelevant to the task at hand.

Summary

The review of the experimental data pertaining to the relationship of drive conditions to reinforcement has revealed several salient features around which the summary can be organized. The first of these is the fact that the stimuli associated with a condition of deprivation are discriminable by experimental animals. An animal can be trained to make a particular response when the only discriminative stimuli available are those that are associated with a particular kind or level of deprivation. But contrary to what might be expected on the basis of Hullian theory, it seems unlikely that the response, in these circumstances, is conditioned to the entire pattern of interoceptive stimulation which is prevailing at the time the response is made. Since it has further been shown that it is not necessary for the stimuli to be reduced by the reinforcer in order to serve as a cue for a response, the data that has accumulated in this area can be interpreted as supporting a concept such as a fractional antedating goal reaction or, synonmously, an anticipatory goal response.

The question of whether or not drive level enters directly into a determination of habit strength has been widely disputed and considerable evidence has been provided by the two camps that are divided by the issue. It presently appears that drive conditions may make a small but appreciable contribution to habit strength if the vigor of the response is measured, but that the contribution appears to be negligible when measures of accuracy are used. One can conclude, then, that drive operations primarily serve a motivational function. This itself is of especial importance, for if an animal is to develop a hibit or learn a reinforcement contingency, it is necessary that it accumulate experience in the experimental arrangement. While it is questioned whether or not deprivation operations will result in an absolute lowering of stimulus thresholds (but see Wagman, 1963), there is little doubt that they vary relative thresholds, that is, they increase the probability that the animal will respond to the relevant stimulus rather than to other stimuli that are irrelevant to the task. By manipulating drive, then, the experimenter can ensure that the animal will be less inclined to engage in competing behaviors.

Consideration of irrelevant or alien drives further complicates the picture. Irrelevant drives were first investigated on a large scale to test the Hullian hypothesis that relevant and irrelevant drives combined additively and multiplied with habit strength to determine excitatory potential. Several investigators found that instead of an increasing monotonic function, there was a curvilinear relationship between response strength and the strength of the irrelevant drive. Thus it is seen that the Hullian postulate suffers serious limitations. At least two considerations can be introduced to account for the conflicting results:

conflicting responses may be conditioned to the competing of alien drive stimuli; and the relative strength of the relevant incentive (the reinforcing stimulus) is diminished, the animal being more highly motivated to obtain a stimulus that is related to the dominant condition of deprivation.

The survey has also revealed considerable data that provide confirmation of the Yerkes-Dodson law, the principle that the optimal drive level for learning a task varies inversely with task difficulty. Since this sort of relationship also has emerged, although with less consistent results, in studies in which measures of generalized drive and stress have been used, the data suggest that the law may be of considerable importance.

The most far reaching concepts that have been advanced to account for these experimental findings are based upon a concept of optimal arousal that varies inversely with task complexity. The most popular interpretation of the mechanism by which this operates is based upon the developing knowledge of the ascending reticular activating system; it is posited that at relatively low levels of activation, the organism is broadly and only generally aware of stimulus change in the environment, but at higher levels there is, in a sense, a concentration of attention on a restricted range of stimuli. At exceedingly high levels of activation, the organism is relatively incapable of grasping complex stimulus contingencies, or of responding appropriately in an experimental situation that makes the appropriate response contingent upon a complex pattern of stimulation.

Chapter 7

Time Conditions Related to Reinforcement

While the typical learning experiment is conducted in such a manner that a response is always and immediately followed by the presentation of a reinforcer, this is neither necessary for learning, nor likely to occur during the normal course of events. The conditions of learning that occur as a result of variations in the time conditions are of theoretical significance, and because of the similarity of these conditions to those that are most frequently encountered outside of the laboratory, they are also of practical consequence. The literature on the topic, which at the present time is rather extensive, covers two areas of experimentation. One condition, delay of reinforcement, is concerned mainly with the time conditions between the performance of the response and the presentation of the reinforcer on each trial, the conditions from one trial to the next remaining essentially the same. The second area is schedules of reinforcement or partial reinforcement, both of which are concerned with varying conditions on successive trials. *Generally, the presentation of* the reinforcer is determined by the prearranged schedule; not every response is reinforced.

Delay in Reinforcement

While it is true that it is not necessary for a response to be followed immediately by a reinforcer in order for learning to occur, this statement is not particularly informative, for it conceals far more than it reveals. Does learning occur at the same level of efficiency if reinforcement is delayed? What are the temporal limits of delay if learning is still to occur? Are there other factors that mitigate the effects of delay? All of these are legitimate and important questions.

So too is the question of what happens when a student submits a completed assignment but has to wait for the results.

Thorndike (1913) showed his cognizance of these problems when he wrote, "other things being equal, the same degree of satisfaction will act more strongly on a bond made two seconds previously than on one made two minutes previously ...(p. 172)." Although it is not specifically stated, the principle implies a gradient of reinforcement, the reinforcing effect of a stimulus diminishing as a function of time. Thus a reinforcer would exercise the greatest reinforcing effect if it occurred immediately following a response, and the effect would diminish as a function of increasing delay between the response and the reinforcement. In spite of the fact that the principle has common sense to recommend it, early experimentation failed to confirm it. Simmons (1924), for instance, delayed reinforcement for an hour and a half with no detrimental effect. In other studies by Watson (1917) and Warden and Haas (1927), no gradient of reinforcement was found over periods of several minutes; the animals that received delayed reinforcement learned the experimental tasks as rapidly as those given immediate reinforcement. This is surprising, especially in light of the fact that the two tasks were quite different. Watson had required the rats to burrow through sawdust to the food source, while Warden and Haas had used a complex maze. But in both experiments, delay in reinforcement was produced by covering the food dish with a heavy perforated cover so that the rat could smell the food but could not gain access to it until the cover was removed. Hull (1943) pointed out that while primary reinforcement was indeed delayed, the experimental conditions provided for strong secondary reinforcement. In this sense, the experiments failed to qualify as having delayed reinforcement.

The importance of secondary reinforcement in the foregoing studies is demonstrated by the fact that in a later experiment conducted in 1929, Hamilton (née Haas) returned to the problem and achieved different results. In this experiment, hungry rats learned a five-stage Warner-Warden Y-maze but were delayed in a special compartment separated by a solid guillotine gate from the food compartment. Unlike the previous experiments, a difference was found in rate of learning between the delay and no-delay groups; although different amounts of delay produced little difference in learning, a gradient was suggested by the data. Since this experiment differed from the earlier ones mainly in the amount of secondary reinforcement that was available, Hull considered this to be the crucial factor in producing the difference in results. Consequently, the experiment conducted by Hamilton produced some confirmation of a gradient of reinforcement.

Another source of data which Hull regarded as crucial to the problem of delay is that obtained by the performance of rats in straight alley mazes. Hull (1934) had observed that the speed of a rat in the first segment of a long alley was appreciably slower than the speed of a rat in the corresponding segment of a short maze. Because of the time required to traverse the mazes, running in the first part of a long alley is reinforced after a longer duration than is the running in the corresponding section of the short maze; thus a gradient of reinforcement also accounts for these results.

On the basis of these and other data, Hull (1943) posited a primary reinforcement gradient that was steep and short in duration, reaching the zero line at a point representing about a thirty second delay time. An experiment which Hull considered crucial to the support of this hypothesis was conducted by Perin (1943) who attempted to design a delayed reinforcement situation in which secondary reinforcements (of the kind considered by Hull) were as far as possible absent. In the Hullian interpretation of the delayed reinforcement experiment in which a rat runs through a maze, enters the delay compartment, and finally enters the goal box, the cues in the passages leading to the goal box are alleged to acquire secondary reinforcing properties because of their temporal relationship to the primary reinforcement. Perin attempted to design an experiment in which such secondary reinforcers were as far as possible absent. Perin changed the delay task from a runway task with a chain of stimuli leading to the reward chamber, to a problem box in which the rat performed a response and was reinforced after a predetermined delay.

Perin devised a Skinner type box in which a rod was set which could be moved a few millimeters either to the left or the right. In the habituation part of the experiment the rat received a pellet of food if the bar was moved in either direction. In the experiment itself, the apparatus was so arranged that a movement of the rod in what had been the preferred direction yielded no food, but a movement of the rod in the non-preferred direction caused the rod to be withdrawn and the delivery of a pellet of food after a timed interval which varied from 0 to 30 seconds. The relationship of delay in reinforcement to learning was such that little measurable learning occurred with the 30 second delay group. The gradient of reinforcement reached zero between 30 and 40 seconds.

In view of this evidence, Hull (1943) posited two active gradients; the primitive gradient of primary reinforcement mentioned above, and a gradient of secondary reinforcement. The secondary reinforcement gradient was much longer (or of greater temporal duration) and was based upon the demonstrable capacity for stimuli associated with primary reinforcement to acquire secondary reinforcing properties. This conditioning presumably developed according to the requirements of the primary gradient; the capacity of a stimulus to reinforce a response was thought to be an increasing function of the temporal proximity of the stimulus to the primary reinforcer.

Shortly after Hull had formalized his position in his Principles of Behavior, Spence (1947) wrote a critical article in which he noted, among other things, that the data that had accumulated on the gradient of reinforcement revealed two important factors. In the first place, the length of the gradient seemed to diminish as a function of increasing experimental control over the sources of secondary reinforcement. Further, and this seemed to him to be of considerable importance, the gradient seemed to be greatly restricted when the greater control was exercised over the consequences of both correct and incorrect responses, a matter that suggested to him that proprioceptive cues had provided sources of secondary reinforcement in previous experimentation. Several studies were cited (e.g. Gulde, 1941; Riesen, 1940) which seemed to support this position. His new interpretation, then, was that the development of habit in those cases in which primary reinforcement was delayed was the result of immediate secondary reinforcement. Grice (1948) generally confirmed this position, for it was found that with a delay of ten seconds, three of five animals were unable to learn a black-white discrimination problem. Even with a five second delay, the speed of learning was greatly retarded.

Hull (1952), in A Behavior System, modified his position considerably in order to accomodate the more recent findings and interpretations. In the first place, he reduced the primary reinforcement gradient to five seconds in order to bring the gradient into accord with the findings mentioned above. Secondly, he distinguished between two types of delay situations and treated them with separate corollaries. The first type of delay is referred to as within-chain delay. A rat that runs through a compound Y maze with five choice points is delayed more in obtaining reinforcement than is the rat that runs down a short straight alley to reach a goal box. The length of the chain of events leading to the reward determines the length of this type of delay. The second situation is that in which the animal, following the correct response, is delayed in a goal box or special compartment where it awaits the forthcoming reinforcement. Finally, Hull now saw that the problem was to explain how a habit could develop when the delay was longer than five seconds. To do this he made greater use of the concept of a fractional anticipatory response mechanism as an explanatory principle.

The anticipatory response mechanism, for our purposes here, is only briefly developed in relation to the performance of a hungry rat in a maze learning situation. In such a situation, the rat entering the goal box faces the stimulus situation sg provided by the food. The response to S_G is the consumatory response R_G. Such a response R_G consists of a complex of responses, some of which actually require the presence of food for their occurrence while others do not. For example, the actual seizing of the food requires that the food be available, but the fractional component involved in salivation may and does occur without the immediate presence of food. The fractional components which can occur without the actual presence of food are denoted by the symbol r_g. When this fractional response occurs it becomes conditioned to traces of cues in the alley leading to the goal box and particularly to those cues nearest to the goal box. Thus the cues in the alley acquire the capacity of arousing the r_g responses prior to the actual entry to the food compartment. The learning process by which r_g becomes conditioned to these cues is described by Spence as a classical conditioning procedure which he interprets as contiguity learning. In addition, the fractional anticipatory response r_g produce interoceptive stimulus cues s_g. The instrumental motor responses become conditioned to these stimuli s_g which thus become a determiner of the locomotor response.

Hull explained within-chain delay in terms of the number of stimulus traces to which the response at various points of the maze could be conditioned. In other words, he felt that the fact that a rat ran more slowly at the beginning of a long maze than at the beginning of a short maze was due to a difference in the strength of the habit that developed in these two situations. Spence (1956), on the other hand, interprets the difference in performance as a difference in motivation. Although he, also, interjects the anticipatory response mechanism to account for the results, he places far greater stress on the motivational component of the construct. In terms of this mechanism, the incentive motivating factor K is believed to be less strong at the beginning of the chain than at the end. The latter is due to the fact that the stimulus cues at the beginning of the maze will be less effective in arousing r_g than will the stimulus cues at the end of the maze. This results in differences in reaction potential (E) in different parts of the maze that are attributable to differences in K. Apart from the scientific elegance of the proposed mechanism, it has a certain plausibility in terms of the problems of teaching. Every teacher hears that when a long assignment is given with a temporally remote reinforcement, the problem is to motivate the students in the early part of the assignment. However, the reader can well justify the position that the chaining of behavior in the maze is remote from the behavior of the child working on a long assignment. The resemblance of behavior in the two situations may be only superficial.

The $r_g - s_g$ mechanism has not been described in completely clear terms and lacks experimental support. It has also come under fire from various quarters and has been criticized by Ritchie (1959) because of certain inadequacies in accounting for discrimination behavior. Nevertheless, it is a notable attempt to account for expectancy behavior within an S-R framework. Th implications of the $r_g - s_g$ mechanism for the understanding of the effect of delay in reinforcement must now be considered.

The second type of delay which involves a waiting period between the completion of the task and the administration of the reinforcement is believed by Spence (1956) to have an entirely different explanation. He suggests that there are two mechanisms operating during the delay period which have opposite effects. First, during the delay, conditions exist similar to those existing during partial reinforcement. The cues in the delay area have acquired secondary reinforcing properties which reinforce the fractional anticipatory responses related to the consumatory response and which also maintain the incentive motivational variable at a high level. But there is also a second phenomenon which must surely occur during the delay period. The delay is comparable to a period of experimental extinction and during this period both the r_g and the related K factor increment will steadily weaken. As this happens the animal tends to make other responses. Spence considers that what the animal does during the delay period is crucial to his position. An understanding of his position requires the introduction of some data. Two studies from his treatise on delay point this out (1956). One carried out by Shilling (1951) required bar pressing for food. He used three matched groups of rats trained on a bar touching response for three blocks of eight trials each with a one-second delay in reinforcement. Following this initial training, one group continued to perform under one-second delay, another was changed to five-seconds delay, and the third group was not reinforced at all. While the third group extinguished following the change of conditions, the one and five second animals did not essentially differ in response latency. At a later point, the one-second control animals were shifted to ten-seconds delay. At this point they maintained (as had the five-second delay group earlier) a posture of attention to the bar and reward mechanism for at least the first five seconds but later in the interval turned away to other inappropriate behaviors. Under these conditions extinction began to occur. Spence therefore held that performance decrement is due, "in part at least," to this loss of "orientation toward the stimulus complex of the response manipulandum and food box."

Other experiments have suggested that the type of apparatus could be used to control the length of time a rat could maintain this orientation. Carlton (1954) set up a study to examine this hypothesis and compared rat performance in a confining chamber against performance in an unconfining one. The expectation was that a confining response chamber in which the rat was not free to move would hinder the occurrence of interfering responses during the delay period.

The confining apparatus contained a starting box plus a response-reward chamber of identical dimensions (6" x 2" x 3" high) placed end to end. The nonconfining apparatus had a much larger response box (17" x 6" x 11 1/2" high). The hypothesis governing the experiment anticipated that under delay reinforcement the animals responding in the confining chamber would be less likely to make incompatible responses and thus lose orientation to the food goal. The animal not confined in this manner would be expected to engage in interfering responses during the delay period. This fact, in turn, would result in a lower habit strength for the correct response (or a higher habit strength for an incorrect response). Using latency of response as a measure of habit strength, the hypotheses were nicely confirmed. The confined and unconfined animals did not differ when reward was immediate, but when reward was then delayed for a period of ten seconds, the latencies increased much more rapidly for the unconfined group than it did for the confined group. When reinforcement was initially delayed, the group that was confined performed at a much better level than the unconfined group, but here, as above, the differences essentially disappeared when reinforcement was made immediate. In both conditions, then, the confined group that was forced to maintain a goal orientation responded more rapidly than the unconfined group.

While the evidence presented above is sufficient to demonstrate the point, additional and more provocative data come from other quarters. Skinner (1948) in his investigations of the effect of delay in the non-chain situation, found that frequently a pigeon will develop his own "superstitious" chain of behavior to mediate between the response and the reinforcement. By using a delay of about fifteen seconds, in some cases, he has found that highly ritualized forms of behavior, such as turning in counter-clockwise circles, will become very stable. Ferster (1953) has made the same sort of observations and also found, as did Skinner, that once a "superstitious" form of behavior has been developed, the length of delay can be increased and the animal will continue to respond at a stable rate.

The fact that a response delay can be increased once a response has been stabilized was also demonstrated in an experiment conducted by Harker (1956). As in other investigations conducted in this area, he used a modified Skinner box which had provision for the removal of the bar. The performance measure used was the mean latency of response after the bar was inserted. The standard control group was reinforced with food pellets after a delay of one second. The experimental group of most interest at the moment was reinforced after one second for the first 40 trials, but from then on reinforcement was delayed for 10 seconds. This group continued to respond as rapidly as the control group. One of two additional groups receiving reinforcement after ten seconds delay during the initial training was subsequently changed to one second delay. This group showed a rather sudden increase in response speed (suggesting a motivational factor as suggested by Spence), and continued to increase to the same level of performance as the control group.

In a provocative study by Pubols (1958) food-deprived rats ran to one arm of a Y maze with delayed reinforcement. During this initial part of the learning experiment, acquisition was slower for the longer delays in reinforcement, a result consistent with other findings. However, our main interest here is in the second part of the experiment in which the reinforcement was switched to the other wing of the Y maze, but with the same delay in reinforcement applied under the new conditions. Pubols found that the extent of the preservation of the old habit and the slowness of learning of the new habit were both related to the amount of delay of reinforcement. Several possible explanations may be applied to this phenomenon. On the one hand the phenomenon fits rather closely the partial reinforcement paradigm for which, of course, there are several possible explanations. On the other hand, the difficulty of learning the new habit may be attributed directly to a difficulty of producing extinction in this particular situation. If one takes as a model of extinction that proposed by Skinner (1950), extinction can be viewed in terms of the novelty of the extinction situation in contrast with the acquisition situation. Since in this particular case the acquisition situation is highly similar to the extinction situation, one would expect the latter process to be slow. Pubols himself offers a very complex explanation which savors of overtheorizing.

Although there has been considerable effort expended in recent years to explain the effect of delay in terms of response theory, in which case it is a response to a particular stimulus configuration that is 'strengthened.' This is by no means the only avenue toward fruitful investigation. Mowrer (1960a), as a particular case in point, prefers to interpret learning as a process of conditioning stimuli (both exteroceptive and response-produced, interoceptive) to the affective component of the response that is made to the reinforcing stimulus (either positive, as food, or negative

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as shock). His major thesis in respect to the problem of delay is that the affective response is conditioned to the stimulus trace rather than to the stimulus per se. He reviewed several experiments that covered the four combinations of the two variables above and cited relevant experimentation to show that it is very likely that a stimulus trace will persist for about 30 seconds, thus setting a temporal limit for the gradient of reinforcement that is longer than that proposed by Hull in 1952. He correctly observed that Hull had based his new gradient of five seconds on data that were accumulated in problems involving discrimination learning, a type of problem that is more complex (thus more difficult) than place learning. But even in the case of discrimination learning, which requires first a process of stimulus differentiation, it seems that the gradient, under appropriate circumstances, may exceed five seconds. Mathers (1953) reported that a black-white discrimination problem produced a gradient with a 20-second duration.

In further support of Mowrer's position Kamin (1959) used a Miller-Mowrer escape conditioning shuttle box to show that a response will be inhibited if punishment is delayed. He trained rats to escape to the non-shocking side of the box at the sound of a buzzer. After training this escape response, the rats were punished only for making the response. If the rat remained in his present position at the sound of a buzzer, shock did not follow. If he made the escape effort, he was shocked either immediately, or after 10, 20, 30 or 40 seconds of delay or not punished at all. Punishment was in all cases response inhibiting, and manifested its effect as a declining monotonic function of delay, analogous in an opposite direction to delayed positive reinforcement. While Kamin points out that his punishment data are consistent with the notion that aversive effects are similar to reward effects but in an opposite direction, his data are not adequate to lend strong support for such a position.

Bourne (1957) studied the effect of delay in reinforcement on the learning of a concept identification task. Human subjects were shown a series of diagrams which they had to classify in four categories. Classification of a diagram was performed by pressing one of four buttons. As soon as the button was pressed the diagram ceased to be visible. Feedback concerning the correctness of classification was given between 0 and 8.0 seconds after the response. Performance on the task decreased as the delay in feedback was increased, but delay did not show an interaction with task complexity. Bourne proposed that the effect of the delay can be best understood in terms of a fading of a stimulus trace.

With human subjects, it would appear, delays of very long duration would be permissible in simple discrimination problems, for the stimulus can be verbally coded and the code will mediate between the actual stimulus and the reinforcement.

Preschool children have demonstrated more rapid discrimination learning when reward is immediate. Lipsitt, Castaneda, and Kemble (1959) trained two groups to discriminate between red and blue. In the pretraining trials, the control group was given immediate reward for either a red or blue choice. It was found that there was no significant difference in speed of response between the two colors. The experimental group was given immediate reward for one color and delayed for the other during pretraining. At the end of training, this group had learned to prefer the color associated with immediate reward. A discrimination test was subsequently given to both groups, in which the immediate reward color of the experimental group now became the correct response choice for all subjects. Each subject was then rewarded immediately for the correct choice. The result was that the experimental group took significantly fewer trials to reach criterion and made significantly more correct responses in the first 20 trials. Probably because of the diminishing trial, discriminations are made more easily when the stimuli to be discriminated are presented simultaneously than if these stimuli are presented one at a time in series. Erickson and Lipsitt (1960) studied children in a 2 x 3 design where one dimension was simultaneous versus successive discrimination, and the other was 0, 3, or 6 seconds delay. The discriminations were to be made to red, orange, and green lights.

Differences between the three delay groups were not significant nor was an interaction between main effects significant. With respect to total number of correct responses, the simultaneous group did not significantly differ from the successive group, but interaction of trials by discrimination type was significant at the .001 level, indicating that the slopes of the learning curves were different. Simultaneous discriminations resulted in more correct responses of 39.3 against the successive groups 34.6. There were 13 subjects in the simultaneous treatment who failed to solve the problem and 22 in the successive treatment.

In another experiment by Hockman and Lipsitt (1961), 60 fourth grade subjects were divided into two groups. One group learned a two-stimulus discrimination problem and the other group a three-stimulus discrimination problem. Rewards consisting of a red light onset were delayed 0, 10, and 30 seconds. It was found that delay of reinforcement had an increasing deleterious effect on discrimination learning but that the effect was greatest with the more difficult problem.

In a similar vein, Sax (1960) found that delay exercised a detrimental effect when high school subjects were given a complex task. His subjects were required to learn the roots of complex chinese characters; those that were given delayed feed-back required more trials to reach criterion than the subjects that were immediately reinforced.

It would seem, then, that learning requires either that reinforcement (either primary or secondary) be supplied within 30 seconds, or, and this is of particular importance with human subjects because of their capacity to symbolize, that the stimulus be in some manner codable so that a verbal (or some other mental) process will mediate between the stimulus and the reinforcement.

But what is probably of equally great concern in an educational setting, is that there seems to be considerable data to support the notion advanced by Spence (1956), namely, that delay results in a decrement in the incentive value of the reinforcer. When all other things are equal, humans prefer immediate reinforcement to delayed reinforcement. Mischel (1961) explored the question of how children would react to the option of an immediate small reward as opposed to a delayed large reward. He hypothesized a relationship of choice behavior to social responsibility. It was his suspicion that less socially responsible children would choose immediate reward more often than children at the other end of the scale.

The subjects for the investigation were 206 Trinidadian negro children aged 12-14. Out of this group 70 came from a boys' industrial school for delinquents and 136 were from a government elementary school (78 boys and

58 girls). A paper and pencil measure of social responsibility was obtained before offering candy to them. They were told that they might choose either a \$.05 or a \$.25 candy bar differing in size and packaging. The small one could be had immediately but the larger one could not be obtained for a week. A separate Trinidadian group of delinquents had previously divided 50/50 in preference for these alternatives.

At the school for delinquents, 31 children chose to get the candy immediately while 39 preferred to wait a week for the larger reinforcement. At the school for non-delinquents, 35 children elected immediate reward while 101 chose delayed (chi square = 6.48, / $p < .02$). Data from this school for non-delinquents demonstrated that children accepting delay have significantly higher social responsibility scores ($t=2.24$, $< .025$). Differences were in the same direction for subjects at the juvenile delinquent school but were not statistically significant. Considering that the delinquent group was a select group which would be expected to perform at the lower end of a social responsibility scale, the outcomes are in accordance with anticipation.

The results to be stressed in the foregoing experiment is that delay does result in a decrement of the incentive value of the reinforcer. In the initial comparison, five cents given immediately was equal in incentive value to twenty-five cents a week later. It would probably be hasty to conclude that this necessarily shows a casual relationship to social responsibility, for Mahrer (1956) demonstrated that preference for delayed reward is a function of the expectancy of occurrence. It does not seem unlikely that the delinquent group would have less expectation that the promise would be fulfilled.

The Partial Reinforcement Effect

The discovery of the partial reinforcement effect is generally attributed to Humphreys, who uncovered it while undertaking a doctoral dissertation. The published report of this study (1939a) states that human subjects were exposed to an eyelid conditioning situation in which the unconditioned stimulus was the usual puff of air on the eyelid. Humphreys was interested in the effect of reinforcement when it occurred in less than 100 per cent of the instances, pointing out that according to what was then Hull's concept of learning, the unreinforced trials should function as extinction trials. If such were the case, then reinforcement in less than 100 per cent of the trials should result in a slower rate of acquisition: such reinforcement, since it enters into the determination of habit strength, should have had no effect on extinction. But what Humphreys found was that on a schedule of 50 per cent reinforcement, acquisition took place as rapidly as at 100 per cent reinforcement. In addition, there were significant differences in extinction rates. The 50 per cent reinforcement group extinguished less rapidly than the 100 per cent extinction group.

In a second study, Humphreys (1939b) demonstrated a similar phenomenon in a learning situation which did not involve noxious stimuli. In this second experiment, the equipment used involved a piece of apparatus now known as a Humphreys' board. This equipment involves a vertical board with two light sources located about three feet apart in the same horizontal plane. The left light is turned on and the subject is asked to guess whether or not the right light would be turned on. During the acquisition series the right light follows the left light in a certain percentage of the cases. In the extinction series, the right light is never turned on following the left light. In this experiment Humphreys found that the frequency of verbal anticipation of the occurrence of the red light tended to stabilize at a frequency approximately the same as the actual frequency. In addition, he found, as he had in his previous study, that extinction occurred more rapidly following a 100 per cent schedule of reinforcement during acquisition then following a 50 per cent schedule of reinforcement. Following the 50 per cent schedule, the extinction curve showed a rise before the typical decline. Humphreys (1940) repeated the experiment with a conditioned psychogalvanic skin response with almost identical findings, including an initial rise in the extinction curve following a 50 per cent schedule of reinforcement.

Since Humphreys (1939b) thought that traditional reinforcement theory could not account for his experimental data, alternative mechanisms were sought. His suggestion was that the phenomena could be accounted for in terms of expectancy. His proposal was that an acquisition series involves the learning of an expectation of reinforcement while extinction involves an expectation of nonreinforcement.

The phenomenon discovered by Humphreys has come to be known as the partial reinforcement effect, usually designated by the letters PRE. A very large research literature has grown up around this effect for two reasons. One is that while not all of Humphreys results have been found to be reproduceable in other studies, the majority, nevertheless, have been reproduced. Reproduceable phenomena are scarce in psychology and hence are always attractive to experimentalists who then study them in relation to new variables. A second factor in the growth of the experimental literature related to the partial reinforcement effect lies in the theoretical issues which it involves. On the one hand, there have been continued attempts to fit it into the reinforcement paradigm, despite the insistance of some that it does not fit. On the other, the "effect" has stimulated many to formulate alternative theoretical constructs.

A decade after the publication of the original studies by Humphreys, Jenkins and Stanley (1950) published a review of the studies undertaken up to that time concerning the partial reinforcement effect. Their bibliography includes 71 items. The studies appearing in the decade following the Jenkins and Stanley review were reviewed by Lewis (1960). The growing interest in the phenomenon is seen in the fact that the Lewis review covers 109 studies, a substantial increase in the output of studies as compared with the previous decade. Since an additional comprehensive review would be inappropriate at this time, the main points made by these reviews will be summarized here.

In the earlier review by Jenkins and Stanley (1950), three essential points are derived from the studies up to that time. These points are the following:

1. With respect to acquisition, the response strength is built up more rapidly with the 100 per cent reinforcement than under partial reinforcement. However, since the differences in response strength produced by the different schedules are small, Jenkins and Stanley are led to speculate that they might reach the same asymptote given a sufficient number of trials.

2. A second phenomenon is that the behavior acquired is generally maintained at a lower level by partial reinforcement than by 100 per cent reinforcement. Here again the differences are small and are not

always statistically significant.

3. Research over 10 years was found to give solid support to the original finding that partial reinforcement produces learning more resistant to extinction than 100 per cent reinforcement. The differences during extinction for the two groups tended to be large and statistically significant. The effect on extinction is much more striking than the effect on acquisition.

The later review by Lewis (1960) was originally made in the hope of finding some explanation of the partial reinforcement effect, but the author admits that this quest was not a successful one. In an initial section of the review entitled "Data" Lewis attempts first to discover the form of the relationship between the percentage of trials reinforced on acquisition and number of trials to extinction. This is generally represented by an inverted U relationship of the sort produced by Grant and Schipper (1952). However, such a curvilinear relationship with the asymptote in the middle range has not always been found. Lewis and Duncan (1956), using human subjects who operated a one arm bandit with a five cent payoff, found that the total number of lever pulls to extinction was an inverse function of the percentage of reward with the 100 per cent reward group extinguishing most easily. Despite the latter data, the inverted U expresses the relationship most usually found. Why the Duncan and Lewis data provide such a remarkable exception is hard to understand. Grant and Schipper suggest that the asymptote for the inverted U should vary with the degree of acquisition and with a higher level of acquisition it should move towards the lower end of the percentage scale.

In subsequent parts of the section of the review devoted to data, Lewis gathers together information from research pertaining to the different variables that might influence the partial reinforcement effect. These will now be considered under the same headings as those used by Lewis.

Successive Acquisition and Extinction. A program which involves alternately the acquisition and extinction of a skill is very similar to the partial reinforcement program itself in which rewarded trials can be considered as acquisition and the unrewarded trials as extinction. When such trials are grouped together in blocks one has a pattern of successive acquisition and extinction. A further modification involves introducing some unrewarded trials into the acquisition series to make it a typical reinforcement series. The studies of this problem which were reviewed by Lewis were conducted in a number of different learning situations, such as straight alley mazes and jumping boxes, and with several different procedures for alternating acquisition and extinction, and the results show lack of any particular trend. With some, the partial reinforcement extinction effect tends to fade with repeated acquisitions and extinctions, but in others no such effect was apparent.

Number of Trials. The Lewis review cites two studies by Lewis and Duncan (1956, 1958) as providing evidence, and probably the most comprehensive evidence, on this point. They used a lever pulling response with money as reinforcement and adult human subjects. The two studies, taken together, provide data with different numbers of acquisition trials and with varying frequencies of reinforcements. They found that both with continuous and partial reinforcement an increase in the number of trials resulted in more rapid extinction. Lewis points out that the effect of a long acquisition series is to establish a stable stimulus pattern, and the longer the acquisition series the more stable the pattern. The more stable the stimulus pattern the greater will be the change when the extinction series begins. Lewis points out that some data from other investigators does not support his own, but he also points out that other investigators had not generally run as long an acquisition series as he and Duncan had.

Punishment. A partial reinforcement situation can occur in the case of punishment and negative reinforcement. Thus in avoidance learning the subject may be punished on some of the trials but not on others in the early stages of acquisition. In escape learning, on the other hand, the subject is punished whatever he does and typically he must perform some instrumental act to turn off the punishment. Here again the punishment may occur on only some of the trials. Lewis cites evidence that in both avoidance learning and escape learning extinction tends to manifest the partial reinforcement effect and tends to be prolonged when the reinforcement is less than 100 per cent.

Interval Between Trials. The evidence seems to point in the general direction that the partial reinforcement extinction effect occurs regardless of the spacing of the trials. Here, as in most other areas of psychology, not all evidence points in the same direction, but the bulk of it supports the latter statement.

Drive. The partial reinforcement effect is sometimes interpreted as a motivational-incentive phenomenon and, hence, studies which relate drive level to the effect have considerable relevance for understanding the phenomenon. Lewis cites two studies in this area, one of which he undertook with Cotton (1957). In this study rats were run with 1, 6, and 22 hours of deprivation and with 50 per cent and 100 per cent reinforcement, but they could find no interaction between drive level and reinforcement level. Lewis also cites a much more complex study, also using rats, by Linton and Muller (1951) which also used two levels of reinforcement, 50 per cent and 100 per cent, but the extinction series was sometimes run under a different drive level from the acquisition series. Linton and Muller found the greatest resistance to extinction when the drive level for the extinction series was the same as the drive level for the acquisition series.

Stimulus Discrimination and Reversal Training. All studies, even those involving the running of rats in a straight alley, require some stimulus discrimination; however, there is some interest in the extent to which the difficulty level of the discrimination involved influences the partial reinforcement effect, but the information available concerning this problem is rather limited. Lewis cites a number of studies, all of which indicate that reversal training is more difficult to undertake after partial reinforcement than after 100 per cent reinforcement. This is further evidence that the habit strength produced by partial reinforcement is stronger than that produced by an equal number of rewarded trials with every trial being reinforced.

Magnitude of Reward. While Lewis reports one of his own studies with human subjects in which no relationship was found between the partial reinforcement effect and magnitude of reward, which in this case was money, in another study by Hulse (1958), who ran rats in a closed alley maze, the larger partial reinforcement effect was found with the larger reward. The differences in the outcome of the two studies could represent species differences, task differences, or a difference between massed and spaced trials. The two experiments differ to such a degree that no integration of the results seems possible.

Delay of Reward. Lewis reviews quite a large number of studies in which the reward was delayed for various intervals of time. The studies reviewed include only research using rats and hence have limited generalizability. Lewis draws the following conclusion:

> "It seems that partial delays of 30 seconds result in increased resistance to extinction; but that partial delays of 10 seconds or less do not. The delays must occur on more than 25 per cent of the trials and when partial reinforcement is combined with partial delay, resistance to extinction is the greatest."

Interpretation of the Partial Reinforcement Effect

The extensive research which has been undertaken with the partial reinforcement effect does not stem from any practical implications which the phenomenon may have. Interest in it stems from the fact that it is well established, occurring across species and across tasks and with a high degree of reproduceability which makes it well suited for experimental study. Understanding of the effect offers the possibility of providing some insight into reinforcement. Most of those who have been concerned with experimentation with the phenomenon have designed their studies around some theoretical issue, but this does not imply that any deep understanding has been achieved. The fact is that the major theoretical issues remain unresolved though research has provided a vast amount of valuable empirical data which will contribute to the ultimate resolution of the issues. A complete discussion of all the explanations that have ever been raised would fill a book and, hence, our discussion here will be limited to the major issues which have been discussed in the technical literature.

1. Expectancy Hypothesis: As Humphreys (1939a, 1939b) viewed it, the notion of "expectancy" is demonstrated when an organism is in a state of anticipation as a consequence of its prior experience in the presence of a given set of stimuli. Continually rewarded subjects come to expect continuous reward. When continuous reward is terminated, expectancy is abruptly thwarted, and extinction of the response is rapid. The subject has learned to expect reward, never non-reward.

In contrast, subjects trained under a partially rewarded schedule come to expect reward on only a portion of the trials. When a non-reward series begins, the anticipation of reward is inseparably associated with the expectancy of some failure. The partial reinforcement effect is considered evidence of expectancy of success after failure. There is virtually no evidence that runs counter to this hypothesis, but therein lies its major weakness. It fits all of the data so well that its scientific value as an hypothesis becomes questionable. At this time one can hardly conceive of data which would run counter to the hypothesis.

The expectancy hypothesis of Humphreys could also be translated into a fractional anticipatory goal response hypothesis of the type that Spence (1956) has developed. Here again the difficulty is encountered that evidence running counter to the hypothesis is almost inconceivable.

2. Discrimination Hypothesis: (Paradigm I) When a series of events of a particular sort is followed by an unharmonious event, such an event is readily discriminated from the series. Likewise, when each of a series of acts is reinforced, but the series is followed by an act

which is not reinforced, the shift is easily discriminated in the series. (Paradigm II) If, on the other hand, a series of acts is only partially and randomly rewarded, a shift to 0 per cent reinforcement will not be so readily discriminated from the series of events preceeding it. The shift in paradigm I is dramatic, while in paradigm II it may be imperceptible until well along in the extinction series.

The discrimination hypothesis has been developed largely by Mowrer and Jones (1945), Grant and Schipper (1952), and Tyler, et al. (1953). The most convincing evidence of the validity of the hypothesis comes from the work of Bitterman and his associates. A paper by Elam, Tyler, and Bitterman (1954) provides particularly important evidence. In this study hungry rats were run on an elevated maze across a jumping gap to the goal box. The goal box was either black or white, the one being related to reinforcement and the other to nonreinforcement. In the training period the rats were run on a 50 per cent schedule of reinforcement. During extinction some of the rats were run to the condition (black or white) related to reinforcement while the others were run to the condition indicative of nonreinforcement. The data showed that the rats run to extinction in the box specifying reinforcement extinguished more rapidly than those run to the goal box signifying nonreinforcement. The authors suggest that when an animal receives no reinforcement in a situation which ordinarily signifies reinforcement that it can readily recognize that a change has taken place which is the essence of the discrimination theory of partial reinforcement. The experiment also runs counter to the secondary reinforcement explanation since the cues which might provide such reinforcement are changed.

3. Secondary Reinforcement Hypothesis: This hypothesis grows out of Hull's system as stated in 1943. It involves the idea that the stimuli provided by the goal box acquire secondary reinforcing properties through their association with the reinforcement provided with which they have temporal contiguity. The secondary reinforcing properties acquired by the goal box stimuli according to this theory have reinforcing effects on the nonreinforced trials. The theory has considerable plausibility, but several lines of research run counter to it. In the previously cited study by Elam, Tyler, and Bitterman (1954), the change in the stimuli in the goal box had exactly the opposite effects of what it should have had according to the secondary reinforcement hypothesis. In addition, while according to the secondary reinforcement hypothesis the same effect should be produced by random reinforcement (50 per cent) as by alternating reinforced and nonreinforced trials, Longnecker, Krauskopf, and Bitterman (1952) have shown that with human subjects and a psychogalvanic response, the random reinforcement group shows greater resistance to extinction than the alternated reinforcement group. This again runs directly counter to the secondary reinforcement hypothesis.

4. Intertrial Aftereffects Hypothesis: This hypothesis was put forward by Sheffield (1949) and forms the basis of her research. She postulated that the terminal activity of each trial was carried over, to some extent, to the beginning of the next trial, at which time it became part of the characteristic stimuli occurring at the beginning of the trial. Through reinforcement, the responses which constitute the trial become conditioned to these stimuli. On nonreinforced trials the stimuli, presumably internal, which result from searching, conflict, and frustration, become part of the stimulus pattern of the next trial

and, hence, may become components of the stimuli to which the responses for the next trial become conditioned. This would account for the fact that extinction is prolonged after partial reinforcement since the stimuli associated with nonreinforcement have become a part of the acquisition stimulus complex. Sheffield argued that if such a mechanism were operating, that spaced trials should prevent aftereffects of a trial being carried over to the next trial. Using a straight alley maze with rats she found that the resistance to extinction of partial reinforcement acquisition did not occur with spaced trials.

While the Sheffield hypothesis is attractive, as Lewis (1960) points out, attempts to reproduce the Sheffield results have not been particularly successful.

An hypothesis related to the aftereffects hypothesis of Sheffield is suggested by Grosslight, Hall, and Murnin (1953). The suggestion made by the latter group of workers is that nonreinforced trials followed by reinforced trials results in the reinforcement of persistance. Such an hypothesis is difficult to evaluate. On the surface it appears to say no more than that subjects thus reinforced will take more trials to extinguish than animals that have not been thus reinforced.

5. Hypotheses Involving the Extinction of Irrelevant Responses. Weinstock (1954, 1958) has attempted to account for partial reinforcement phenomena in terms of the extent to which partial and 100 per cent reinforcement provide conditions for the dropping out of irrelevant components of the response chain. Any task learned through a sequence of responses which lead to reinforcement include some irrelevant responses which may, at times, interfere with the relevant ones. Weinstock suggests

that on unreinforced trials the irrelevant responses, presumably because they are weaker than the relevant responses, may drop to strengths below threshold level. The suggestion is that this permits them to drop out. The result is that during the extinction series there are fewer competing non-goal directed responses to disrupt the chain already established. The argument is not too convincing since it makes assumptions about the relative strength of relevant and irrelevant responses which are difficult to justify.

6. Frustration and Failure Tolerance Theories. This hypothesis has been most fully developed by Amsel (1958) and his students who have worked on it over a period of years. This hypothesis, as it has been developed by Amsel, involves the use of the fractional anticipatory goal response mechanism as it has been developed by Kenneth Spence in his more recent writings. Amsel takes the position that all goal events can be classified as rewarding, punishing, or frustrating. With these three types of goal events are associated three classes of anticipatory responses, r_g--s_r, r_g--s_p, and r_g--s_f, in which the subscripts r, p, and f, refer to reward, punishment, or frustration. According to this viewpoint the r_g--s_f mechanism has motivating properties which result in greater vigor of behavior when it occurs. Amsel, et al. discuss the operation of the r_g--s_f mechanism as the frustration effect. A convincing demonstration of this effect is given in an experiment which they cite. This experiment involved a start box, a runway (R1), a goal box (G1), a second runway (R2), and a second goal box (G2). In the experiment hungry rats ran to the first goal box where they were either rewarded with food or unrewarded, and then they ran to the second goal

box where they were rewarded. The frustration hypothesis states that after nonreward in G1, the r_g--s_f mechanism involved in frustration should raise the level of motivation on the second runway and produce a faster running rate. This is exactly what happened. The inference is made that in partial reinforcement, the nonrewarded trials have r_g--r_f components which raise the level of arousal during acquisition. This heightened level of arousal then appears during the extinction series.

Schedules of Reinforcement

In the last few pages, attention has been given to the condition of partial reinforcement and its effect on learning. The typical partial reinforcement condition, with reinforcement occurring on a fixed percentage of trials and distributed randomly, is a special case of the more general set of conditions known as schedules of reinforcement. It is very obvious that the particular arrangements of reinforcements commonly found in partial reinforcement studies represent only a few of many possible arrangements. Others are also commonly found within the framework of partial reinforcement research of which one of the commonest is to alternate reinforced and nonreinforced trials.

This section on schedules of reinforcement was introduced with a review of research on partial reinforcement because of the intensive effort which has been devoted to that particular reinforcement contingency and the concentrated effort that has been made to find an explanation for the phenomena which have been revealed. The historical development of ideas and experiments concerning the effects of the schedule of reinforcement also began with partial reinforcement through the early work of Humphreys. This fact also supports the approach which we have taken. Unfortunately for the historical reviewer, ideas do not always develop in a logical and orderly way.

An almost limitless number of combinations of reinforcing conditions are possible, and no doubt a great number operate within the context of everyday human experience. A large mass of data collected in laboratories is available to indicate the effects of such schedules, but the ordering of such data appears to be an immense task. Hopefully though, there is the concomitant impression that parametric laws to define the operating effects of reinforcing conditions are not only possible, but perhaps some of these laws are not far from being formulated.

At present there are hundreds of laboratory studies which indicate the effects of various reinforcement schedules for limited single variable conditions. Knowledge is much less replete with multiple variable conditions, and very sketchy with regard to shifting conditions of reinforcement. In spite of the abundance of data, there remains a paucity of lawful ordering. A great deal of interesting work proceeds without evidence of an interest in defining parameters.

Logan's work typifies the kind of continuous effort which is most promising for opening up an understanding of the effects of multiple conditions related to reinforcement. At Yale he has carried out a continuing program of rat studies in order to achieve some kind of lawful ordering of fixed and variable reward size, delay of reward, combinations of continuous and intermittent reward, correlated reinforcement and other reward contingencies. Several years of work have been summarized in his book, _Incentive_: _How the Conditions of Reinforcement Affect the Performance of Rats_ (1960). Some of the outcomes he relates will be reviewed in other parts of this monograph.

Discontinuous or partial reinforcement schedules have been studied in a variety of ways. Ferster and Skinner's work, Schedules of Reinforcement (1957), was an exhaustive effort to identify possible schedules. They list four major types of partial reward schedules (these have been broadly accepted as the major schedule breakdowns by other experimenters) which in turn have been divided and combined to result in thirteen types of reinforcement schedules. When the time interval between reinforcement is constant, the schedule is identified as a fixed interval (FI). By varying time between reinforcements, we identify another: variable interval (VI). The remaining two schedules, fixed ratio (FR) and variable ratio (VR) are determined by a ratio of responses to reinforcements. In the case of the fixed ratio, the reinforcement is given after a constant number of responses. The variable ratio schedule, on the other hand, is one in which the number of responses required before a reinforcement is given varies from one occasion to the next. For instance, a reinforcer may be given after 10, 20, or 30 responses in random order. These schedules are defined by the mean number of responses required and by the range of ratios used in the schedule. Research with schedules of reinforcement has become extremely popular during recent years, mainly because of the consistency of results obtained in this sort of experimentation. Each schedule of reinforcement produces a characteristic pattern of response that is reproduceable in many different experimental arrangements using a variety of species. Generally, response rates increase with the frequency of reinforcements, the rates being roughly proportional to size of the ratio. Conversely, more responses per reinforcement are obtained with smaller ratios of reinforcement.

It has been shown time and again that fixed ratio and fixed interval schedules, both of which are situations in which the presentation of the reinforcer is somewhat predictable (or regular), are conducive to the development of a high degree of variability in response rate. Typically, pauses will occur after the presentation of a reinforcer, and following the pause, the rate will gradually increase until the presentation of the next reinforcer. This results in what has been termed scalloping for this describes the appearance of the curve obtained when the number of responses is expressed as a function of time.

In contrast to fixed schedules, schedules employing variable intervals or variable ratios result in a regular rate of response, the responses being more evenly distributed throughout the period of experimentation. This is believed to be due to the fact that the organism is not able to discriminate the conditions under which reinforcers are administered. Indeed, the conditions do not remain the same throughout the experimental session. Variable interval schedules result in a lower rate of response than variable ratio schedules, for it is possible for the animal to earn more reinforcements by working at a faster rate on a variable ratio schedule. All intermittant schedules, assuming that reinforcements are sufficiently frequent to maintain behavior, result in an increased resistance to extinction, an effect that was covered in greater detail in the section on partial reinforcement.

Most of the studies in the area of schedules of reinforcement have been conducted with rats and pigeons, but important experimentation has been conducted with humans to extend the general findings. Orlando and Bijou, for instance, studied the performance of retarded, institutionalized children and young adults (I.Q. 23 to 64; C. A. 9 to 21) on a task in which the child squeezed a two-piece handle which was connected to a device providing small candy reinforcements via a chute in front of the subject. They observed that the response characteristics produced by the various schedules were just as distinctive with these subjects as with infrahuman subjects, although there was perhaps greater variability among these subjects than among the typical laboratory animals. Further, they found that the use of candy for reinforcement was sufficient to maintain a constant level of motivation throughout the experimental session, at least to the extent that the duration of sessions and inter-session intervals had little effect on the rate of response.

While there is striking similarity between the performance of rather severely retarded children and infrahumans, the similarity diminished in a comparison involving normal children. Long, Hammock, May and Campbell (1958) reported the effects of three schedules (fixed ratio, fixed interval, variable interval) upon the operant behavior of 200 normal children. Trinkets, pennies, and projected pictures were used as reinforcers. One readily observable difference to be seen between retarded and normal children is that these simple reinforcers seem not to maintain their incentive value with normals as well as they do with retarded children. Performance deterioration had to be checked by doubling the penny or trinket reward. This appeared to deter the slow down which otherwise occurred. A more powerful incentive was the addition of projected pictures to the reinforcers already in use. When pennies or trinkets were presented, the picture changed to a new scene. With these incentives under fixed interval schedules, there were regular, post-reinforcement pauses, and high terminal rates with most subjects.

Orlando and Bijou choose to account for the differing responses to incentives between their retarded subjects and the normal subjects on the basis of histories of deprivation and types of reinforcers used.

In spite of the motivational problem, Long, et al. maintained that in most instances children perform in a similar fashion with respect to schedules as do other organisms. They claim that "almost as much experimental control can be gained over the behavior of children as that of lower organisms." However, in the instance of the fixed-interval schedule particularly, the data were not so lawful as with infra-humans.

Almost endless variations of studies are possible in research on the effect of schedules of reinforcement on behavior. One variation is to study the effect of changing the schedule. Reinforcement in which two or more schedules alternate is referred to as a multiple schedule. While Bijou and Orlando (1961) have conducted some preliminary work with multiple schedules, no definitive results have been obtained. Just where such research will lead is difficult to say at this time.

Implications of Research on Schedules of Reinforcement for Education

While research on schedules of reinforcement has been substantial and, at the infra-human level prolific, the implications of the data for education are not, for the most part, clear. The central problem investigated in research on schedules is that of the effect of the conditions on the maintenance of behavior while the central problems of education are focussed on the acquisition of behavior. While the conditions under which behavior is maintained are of significance to education when the long term effects of training are to be considered, the schools can exert little influence on their pupils after graduation when the major problem of maintaining behavior is found.

There are, of course, some problems of maintaining behavior during the school years. The maintaining of attention to the learning situation is one with which educators have long been preoccupied. The general solution to this problem has generally involved the choice of content which commands the attention of the learner. Another approach is suggested by research on schedules of reinforcement.

The major finding with educational implications of immediate importance is that a variable amount of time between rewards and a variable amount of responding between rewards produces consistent work output. This finding is of interest in view of the fact that the typical school practice is to provide rewards at the end of a fixed interval or after a fixed quantity of work has been undertaken. Thus grades are commonly given after a given number of weeks of the semester have gone by or after specific assignments have been completed. This schedule of reinforcement after fixed intervals or after fixed outputs of work fits mass education procedures and simplifies planning on the part of the teacher. The emphasis placed by the progressive education movement on informal evaluation procedures and the dropping of systematic grading procedures would appear to lead to practices much more consistent with what is known about schedules of reinforcement than do the more typically found procedures.

Summary

The research literature concerning time conditions related to reinforcement fall into two categories: first, there are those studies concerned with problems related to the effect of delay on reinforcement;

second, there are those related to the scheduling of reinforcements.

The study of the effect of delay has had a long history and has been the focus of several important theoretical issues. Some early experiments showed a marked gradient in the effect of reinforcement as delay was introduced while others did not. The key to the differences in the results appears to lie in the suggestion that some learning situations include more powerful secondary reinforcers than do others, and these operate during the period of delay.

Hull (1943, 1952) elaborated the secondary reinforcement interpretation of some of the previous studies and proposed a short and steep primary reinforcement gradient and a secondary reinforcement gradient which was much longer in its duration. The consideration that, in the learning of a maze, the reinforcements for the responses in the early part of the maze were delayed much longer than those near the goal box, led him to explain the secondary reinforcement gradient in terms of the concept of a fractional anticipatory goal response.

The reason why the rat runs more slowly in the early part of the maze than in the late part is a matter of controversy. While Hull has interpreted it in terms of a difference in habit strength, Spence has been inclined to believe that the phenomenon is produced by differences in motivation.

The case of delay introduced at the goal may involve a different mechanism from that involved in delay in reinforcement in the running process itself. Spence suggests that the former involves two mechanisms. One is the familiar fractional anticipatory goal response mechanism and the operation of secondary reinforcers. The other is an extinction phenomenon.

Empirical evidence demonstrates that what the animal does during the delay period is crucial. A confining apparatus which limits the responses which the animal can make increases the effectiveness of delayed reinforcement.

Once a response has been established, delay in reinforcement can be increased while still maintaining the response.

Mowrer (1960a) has discussed at length Hull's interpretation of the delay in reinforcement phenomena and has proposed a more elaborate mechanism. He has pointed out that Hull based his position on experiments involving discrimination learning. Mowrer points out that if evidence collected within the framework of place learning leads to somewhat different conclusions, largely because place learning presents a simpler task than does discrimination learning. The essence of the Mowrer interpretation is that an affective response is conditioned not to a stimulus *per se* but to a stimulus trace and he takes the position that a stimulus trace may persist as long as 30 seconds. Thus the gradient of primary reinforcement, set by Hull as reaching a zero value after about five seconds, is extended to a period of about 30 seconds.

Research on delay in reinforcement in children has provided data similar to that derived from subhuman organisms. Humans learn more rapidly with immediate reinforcement and prefer the immediate reinforcement condition. The effect of delay in human subjects is probably related to the extent to which the relevant information about the environment can be coded and stored. There is also some evidence that delay results in a decrement in the incentive value of the reinforcing event.

The partial reinforcement effect, the discovery of which is generally attributed to Humphreys, has been the focus of extended theoretical debate. Acquisition has been generally found to be more rapid with 100 per cent reinforcement than with a lower percentage of reinforced trials, though there are studies which provide exceptions. Behavior is generally maintained at a lower level by partial reinforcement than by 100 per cent reinforcement, although, again, there are studies which provide inconsistent data. The most solid and consistent finding is that partial reinforcement provides learning more resistant to extinction than 100 per cent reinforcement. The latter finding occurs both with positive reinforcement and in avoidance learning situations.

The theoretical issues which arise in the interpretation of PRE remain largely unresolved. Humphreys originally advanced the expectancy hypothesis which, like all expectancy hypotheses, can be translated into a fractional anticipatory goal response hypothesis by those who prefer to think in *terms of S - R concepts.*

Others have preferred to maintain a discrimination hypothesis which proposes that animals have more difficulty in discriminating between acquisition and extinction under partial reinforcement than under 100 per cent reinforcement. A third hypothesis suggests that PRE can be accounted for in terms of secondary reinforcing characteristics acquired by the goal box. The fourth major hypothesis is that internal stimuli associated with frustration on nonreinforced trials become a part of the stimulus complex to which running behavior in a maze becomes conditioned. A related hypothesis is that PRE involves the reinforcement of persistance. Another proposal is that the PRE involves a situation favorable to the extinction of irrelevant responses. A final hypothesis suggests that under partial reinforcement the organism learns to tolerate frustrating situations.

While extensive work has been undertaken on the effect of different schedules of reinforcement on acquisition, on the maintenance of a behavior, and on extinction, the generalizations to be derived from such research are rather few. The fact that a variable amount of time or a variable number of time between reinforcements produces a consistent rate of responding is well known, as is the fact that fixed ratio or fixed time schedules produce variable behavior and scalloping. That similar results are achieved with young human subjects is also of interest. Some workers who have studied children report that behavior is as well controlled by the schedules with such subjects as it is with subhuman subjects. However, the implication of research on schedules for the practical control of human behavior are not impressive.

Chapter 8

The Characteristics of the Reinforcing Event

The reinforcing event, normally conceived of as the reward or incentive in the typical learning experiment, has necessarily been discussed at great length in the preceding chapters. The studies reviewed have utilized reinforcers with distinctively different qualities; thus such divers things as food, water, light, sucrose, saccharin, and novel stimuli have served to reinforce behavior. Not only have the events employed differed greatly in their qualitative characteristics, but these differences have been stressed and have to a great extent provided the foundation for the organization of the earlier chapters. Further, it has been shown that particular characteristics of the organism enter into the reinforcing efficacy of these various stimuli; food is not reinforcing, it has often been said, for a satiated rat. Nor is a rat (or any other creature of the world) likely to be too enchanted with a novel object when he is dying of thirst. But although reinforcers and reinforcing contingencies have been discussed, they have to an even greater extent provided the ground against which other variables have been observed. For the present, then, instead of a complete change in substance, a change in emphasis will suffice for the purpose of explicating the role of the reinforcing stimulus.

As previously noted, reinforcing stimuli or incentives show a vast range of qualitative differences. But even though the differences are qualitative, it typically will seem that some reinforcers are more similar to one another than are other reinforcers. For instance, sunflower seeds and food pellets are similar in more respects than are either of these to light, although all are, under appropriate circumstances, reinforcing stimuli. Crespi (1942) found it beneficial to distinguish between intermodal and intramodal variations of incentives. Intermodal variations, according to the usage provided by Crespi, refer to variations that entail a change in the relevant drive. To a large extent we have been concerned previously with this type of variation in incentive. But little time has been spent in an exposition of the intramodal variations, the changes in incentive that do not entail a change in drive. Sunflower seeds and food pellets constitute intramodal variations, going back to the example of a moment ago, for both will serve to reduce a hunger drive. But as Elliot (1928) has shown, different behavioral consequences will issue from the use of the two. The number of studies comparing intramodal variations of this sort are few (but see Simmons, 1924; and Ligon, 1929), mainly because of the tremendous gap that continues to separate them. More studies have been conducted, however, in cases where the incentive value has been quantified in terms of the results of preference tests. It might be recalled that this technique was discussed at greater length in chapter two. Hutt (1954), for example, found that the rate of bar pressing on a periodic reinforcement schedule was linearly related to the preference value of the incentives used to reinforce the response. Guttman (1954) further demonstrated that bar pressing was related to the sweetness of the reinforcer rather than the nutritive value of sucrose and glucose solutions. But if the reinforcers cannot be related in some manner, if they do not have some characteristic in common, then the work is more fitting an encyclopedist, for the results cannot be integrated or summarized in terms of few concepts. Every incentive would have to be investigated independently. The most popular recourse, as is very apparent after a survey of the experimental work conducted in the area, is to vary the quantity of some particular reinforcer. But to concentrate solely on those studies in which quantity was manipulated would be to neglect some of the more interesting results that have accrued in the experimental investigation of incentives.

Another matter that deserves some preliminary consideration is the distinction that was drawn earlier concerning positive and negative incentives. In many situation, it makes little difference whether an animal learns a response in order to gain a positive incentive, or learns it in order to avoid a negative stimulus, normally a shock. Yerkes (1907) used the latter procedure almost exclusively in his work for he found that the consequences of shock could be controlled far better than those issuing from manipulations of periods of deprivation from food and water. His work, as well as the work of several other investigators (e.g. Cole, 1911; Dodson, 1915, 1917; Yerkes and Dodson, 1908), indicated that the relationship between shock intensity and performance variables such as speed and latency could be expressed as a simple increasing monotonic function. There has been little work in more recent years that necessitates a change in this interpretation, but the reader is cautioned that experiments employing shock have not been as varied as those involving positive incentives which serve to complicate the relationship between magnitude of reward and performance. Mainly because of the lack of experimentation with negative incentives, then, the present discussion will deal only with positive reinforcers.

Size of Reward in Relation to Vigor of Response

The earliest experiment to vary the amount of reward was one conducted by Grindley in 1929. In this experiment, young chicks were run in a straight alley to a food tray in which there was either 0, 1, 2, 4, or 6 grains of boiled rice. The results of this study, which has been widely cited but severely criticized by Crespi (1942), suggested that rate of learning, as measured by the time taken to reach the tray, was positively related to the amount of reward. A study by Wolfe and Kaplon (1941) which was in many respects a replication of the study by Grindley, also used chickens in an alley. In this case, the chickens were reinforced with either a quarter, four quarters, or a full grain of popcorn. The chickens running to a full grain of corn did not run reliably faster than those running to a quarter of a grain. Those running to four quarters, on the other hand, ran faster than those running to one quarter and somewhat faster than those running to whole grain. These studies, more than anything else, should serve to point up some of the difficulties involved in research on amount of reward, for the variables can be quantified in many different ways. Two alternative explanations have been tendered to account for the results obtained by Wolfe and Kaplon. The explanation they favor is that four quarters resulted in greater consumatory behavior on the part of the animal than the single kernel. Crespi, on the other hand, suggests that the differences may be due to the fact that four quarters are more readily discriminated. Certainly if an animal is unable to discriminate various quantities, it is unlikely that it will be able to respond differentially to them.

The inconvenience of using chicks and their unavailability in most laboratories resulted in many later workers turning to the use of rats in a straight alley maze. The typical situation involves the use of a straight alley around four feet long with a starting box at one end and a food box at the other. The typical measure of learning in this situation involves the measurement of the time taken for the rat to traverse the alley. Occasionally other measures are used in this situation. For example, Crespi (1942) used a measure which he referred to as latency and which involved the time taken for the rat to traverse the opening in the starting box. The empirical results obtained with the use of this type of equipment are clear, and a positive correlation was found between running speed and amount of reward in studies by Armus (1959), Bower and Miller (1960), Bower and Trapold (1959), Cotton (1953), Furchtgott and Rubin (1953), Furchtgott and Salzberg (1959), Hulse (1958), McKelvey (1956), Kraeling (1961), Logan, Beier and Ellis (1955), Metzger, Cotton and Lewis (1957), and Pereboom and Crawford (1958). Similar results were found by Dufort and Kimble (1956), and Zeaman (1949) using an elevated maze rather than the straight alley type. The general outcome of these studies is the same despite considerable variation in the measure taken as size of reward. Some investigators have varied the quantity of the food provided which can be done conveniently when standard pellets are used. Others have varied concentration of sucrose in solutions in order to change, presumably, the extent to which the relevant taste bude are stimulated. Still others have controlled the time premitted for the consummatory response. Regardless of the measure involved it is positively correlated with running time and, in the case of the Crespi study (1942), is positively correlated with his measure of latency.

Some understanding of what is involved in the lower running time associated with the larger reward is provided both by Cotton (1953) and by Pereboom and Crawford (1958). These research workers found that a short running time could not be completely accounted for merely by the factor of increased speed of running as such. What appears to happen is that with the larger reinforcement there is less irrelevant behavior and the animal, rather than being distracted by irrelevant stimuli, goes directly to the goal. This effect, it may be recalled, is essentially the same as one of the effects of increases in drive.

Similar findings have been reported in the case of bar-pressing responses in rats. Thus Guttmann (1953) found that rate of bar pressing in rats was generally positively related to the concentration of sucrose in solutions used to reinforce rats which had been deprived of water. In another study by Hutt (1956) the rate of bar pressing in rats was found to be positively related both to the quantity of the substance used as a reinforcer and whether saccharin or citric acid had been added to it. Study by Levin and Forgays (1959) provided a light following bar pressing which under some conditions is known to operate as a reinforcer. In the latter study, the animals were divided into a young group and an old group and reinforcement was provided either by a dim light or a bright light. The findings were that in young animals a dim light was the most effective reinforcer and the bright light actually had a depressing effect on bar pressing behavior. In the older animals intensity of light was found to be unrelated to rate of bar pressing but it was found to have reinforcing properties. In the case of the pecking behavior of pigeons placed in a typical Skinner-type box, the results of research are inconsistent with those previously discussed. Keesey and Kling (1961) could find no relationship between rate of pecking and the amount of reinforcement. Pyron and Wyckoff (1961) on the other hand could find no relationship between the duration of reward and rate of pecking, but the rate of pecking was a linear function of the number of reinforcements.

By and large, the results reported to this point conform very well with the general law stated by Katz (1937). His general formulation stated that the incentive value of a constant reward increased as a function of hunger, and given a certain level of hunger, the incentive-value increased as a function of the amount of food. This effect is not simply limited in application to those situations in which a measure of response vigor is recorded, for differential performance has been also recorded in tasks calling for other response measures. Tinklepaugh (1932), Nissen and Elder (1935), and Cowles and Nissen (1937) found also that in a delayed reaction situation, a larger reward permitted a longer delay. The experimental procedure followed was that of placing a reward (portions of banana and orange) in various quantities under one of two cups. After a period of delay, the chimpanzee was permitted to respond in order to obtain the food. The dependent variable was the percentage of correct choices on the initial response. Cowles and Nissen further explored a question raised earlier and found that the differential reinforcing effect of varying quantities is partly a consequence of having previously received varying amounts in the cup.

Contrast Effects

The suggestion has been made that an important condition for large versus small rewards having an effect on performance is for the same organism to work with both, a condition which would permit a comparison of the one with the other and hence for discrimination to occur between the levels of magnitude of the reward. Schrier (1958) conducted an investigation with monkeys in which some monkeys were exposed to both large and small food rewards, but in different problems, while others were provided with only a single level of reward. Several single reward groups were set up to permit the comparison of reward size on performance in a problem-solving situation. More effect was produced by shifting the reward in the same subjects than was found in comparing two groups, given different rewards. Meyer (1951) had at a much earlier time arrived at the same position through a research from which he concluded that "a major determinant of the facilitative characteristics of a given amount of reward is the range of rewards offered for the performance of a given task." That the factor involved is the capacity of the organism to discriminate between the large and the small reward is brought out by a study of Lawson (1957). In the latter study albino rats were trained in discrimination problems. Four groups were trained. One group was given high reward on both problems and one group was given low reward on both problems. The remaining two groups were given high reward on one problem and low reward on the other in counterbalanced design. The results of this study were that the absolutely rewarded groups did not differ from one another in the number of errors made in the discrimination task. However, the differentially rewarded groups made significantly fewer errors on their respective high reward problems than on their low reward problems. Thorndike (1935) also involved this concept to explain some contradictory results obtained in his laboratory. The experiments under consideration utilized a typical Thorndike format for the conduct of experiments. Connections had to be learned between particular situations and particular responses. In a study conducted by R. T. Rock (1935) in this series, subjects had to learn a connection between a word in code and a corresponding English meaning. The subjects were required to guess which of four English words represented the correct translation of each one of a set of code words. In another situation, subjects learned that they were given the best rating for a ball-tossing skill when they sat on a particular stool. In yet another experiment which Thorndike himself undertook in cooperation with Forlano (1936), subjects learned to match English words with Spanish words they did not know. For each Spanish word, the subjects had to choose one of five English words. In each one of these two experiments a correct choice was followed by the announcement "Right", plus a small reward of money which varied from one cent up to eight cents. In interpreting the results of the experiment, one must keep in mind that they were conducted in the depths of the depression years, when money represented a very important and meaningful reward. The differences in the rewards offered were of real consequence to those who received them.

In the case of the Rock experiment, no significant differences were found between the different levels of reward in their effect on learning. On the other hand, in the case of the Thorndike and Forlano results, a distinct relationship was found between the amount of monetary reward and the amount of learning. An increase in reward resulted in an increase in the rate of learning, except that the highest level of the reward resulted in a slight decline instead of a further increase. Thorndike interpreted the latter result as being due to the excitement of the subject in the high-reward situation. It may be recalled that the relationship between motivation and task performance was discussed at great length in Chapter 6. Suffice it now to say that Thorndike's interpretation has additional experimental support.

How did Thorndike explain the differences in the results of the two experiments? The tasks involved are sufficiently similar that the results are not likely to reflect task differences. Thorndike suggested that the differences are attributable to a slight difference in the experimental procedure. In the case of the Thorndike and Forlano experiment, each subject went through a number of experimental runs and the reward was varied from run to run. Subjects were thus in a position to compare one reward level with another and perhaps modify their behavior accordingly. In the case of the Rock experiment, such a comparison could not be made by the subjects. There is a possibility that a contrast effect is necessary before much differential reaction to different levels of reward becomes apparent.

Adaptation and Expectation

The importance of varied experience on the part of the experimental animal has been mentioned several times to this point, in terms of learning to discriminate varying quantities, in learning the consequences of the difference in amount, and most recently in terms of a "contrast" effect. There is another matter of equal if not greater importance that warrants close attention. It is best illustrated by the results of an experiment conducted by Tinklepaugh (1928). Tinklepaugh frequently utilized a delayed-reaction design of the type mentioned above. Briefly, he would, in the sight of the experimental animal, place an incentive under one of two cups. After a period of delay, the animal was free to respond to the cup of his choice. Hopefully, the animal would remove the cup under which the incentive was placed. Of particular concern here is the consequences of a slight change in the game. It is important to note at the outset, that by the rules of the game the use of lettuce as an incentive for the chimpanzees was a perfectly acceptable practice. Upon finding the lettuce under the cup, the chimp would walk away with the lettuce in his mouth. But when the chimp had observed the experimenter place a banana under the cup and found lettuce in its place the chimp would look around and under the cup, in, under and around the other cup, and under and around himself. He would look and shriek at the observers and finally walk away, leaving the lettuce untouched. There is every evidence here of the chimpanzee having evaluated the lettuce not in terms of its intrinsic characteristics, but in terms of his expectations in the situation.

Crespi (1942, 1944) has been most responsible for calling attention to these presumed affective factors as important variables in the determination of the incentive value of a reward. In his own work, he trained rats in a maze running situation with different groups run with different quantities of food as a reinforcement. After the rats were considered to have reached what was believed to be their asymptote of performance, the low reward group was shifted to a higher level of reward and the high reward group was shifted to a lower level. Crespi found that there was a rapid change in the asymptotic level of performance, but he also found that the low reward group moved to a higher level than the group trained on the higher level reward in the first place. He referred to this phenomenon as the elation effect. In a similar manner the high reward group shifted to a lower level also tended to overshoot the mark and to move to a lower level than was expected. This was termed the depression effect. The change in response as the reward was changed indicates that the animals were able to discriminate the magnitude of the reward. Collier and Marx (1959) found similar depression and elation effects in a bar-pressing situation which would suggest that the phenomenon may have some generality. In their experimental arrangement, three groups of rats were given magazine training in a Skinner box with 4, 11.3 or 32 percent solutions of sucrose being used to reinforce a response to the magazine. The reinforced training was conducted on an aperiodic schedule of reinforcement with 20 reinforcements being given during each 20-minute session for a period of eight days. During the second phase of the experiment, the rats were trained to press a bar in order to gain access to the magazine. In this part of the training, the rats were all reinforced with an 11.3 per cent solution of sucrose. In this fashion, then, one group received a stronger solution, one a solution of the same concentration, and the remaining a solution of a lesser concentration than that presented during the first phase of the experiment. Interestingly enough, the asymptotic performance of the group initially trained on the four percent solution was greater than the rate of the other two groups. The rats that were initially trained on the 32 percent solution, on the other hand, responded at a rate that was significantly lower than that of the rats that were trained on the 11.3 percent solution during both phases of the experiment. These effects, just the reverse of what is normally found with variable concentrations of sucrose, persisted for the ten days of the experiment during which the bar pressing was reinforced and the differential effect was further evident during four days of extinction.

Since the effect persisted for such a long period of time, Collier and Marx interpreted the finding in terms of adaptation instead of elation and depression. They considered that any rat should get over an emotional reaction during the course of two weeks.

There have been similar effects recorded that to some extent escape interpretation without again interjecting an affective variable. In his studies on shifts in magnitude of incentive, Crespi (1942), observed that animals trained with one and to a lesser extent those trained with four units of incentive started manifesting signs of frustration (e.g. jerking, biting, jumping, etc.) beginning about the twelfth trial. Rats that were shifted from 16 units to one and four units also started to manifest signs of frustration after about four trials with the new incentive. But the animals that were shifted to a condition of no incentive showed little evidence of frustration. This qualitative observation is buttressed by the quantitative data derived on the running speeds of the animals. The animals receiving one unit of incentive were significantly (P=.05) slower than the animals receiving no incentive at all. Grindley (1929), who ran all of his subjects to a relatively small reward, observed that fully a quarter of the chickens developed a dislike for rice.

Although the similarity may be no more than superficial, two other studies show that the effect is modified by a number of conditions, if for no other reason than to indicate the sort of data that remains to be explained. The first is one conducted by Guttman (1953). This study has already been cited as one supporting the general position that the reinforcing effect of sucrose varied directly with the concentration of the solution. While this is true in terms of rate of acquisition of the response and extinction, Guttman found a curvilinear relationship between the asymptotic rate of bar pressing and the concentration of the solutions when the rats in the study were continuously reinforced. The rate of response with a 32 percent solution was appreciably less than the rate elicited by a 16 percent solution. On a periodic schedule, however, rates were directly related to the concentration.

In the second study, one employing rhesus monkeys, Conrad and Sidman (1956) found with lever pressing on a variable schedule of reinforcement the response rate was related to the sucrose concentration in the solution used as a reinforcer. However, this relationship did not hold over the entire range of sucrose concentrations. When the latter exceeded 30 percent the response rate tended to decline. The authors of the study suggest that the decline might be due to a satiation effect. Even monkeys can have too much of a good thing.

Magnitude of Reward in Complex Learning.

Most of the studies that have been considered up to this point have involved learning at a very simple level and at a much simpler level than is of concern in most human learning situations. Of particular interest to those engaged in education are studies in which measures of the magnitude or desirability of a reward are related to the number of trials required for reaching a particular level of proficiency in a discrimination learning task.

The relationships of measures of discrimination learning to measures of the magnitude of reward vary from study to study perhaps as a result of variation in technique. When rats are used as subjects a common technique is to use either a T-maze or a Y-maze. In some studies the position of the goal box is learned, but in others the animal learns to discriminate visible characteristics of the goal boxes. The difficulty of the task may be varied by varying the extent to which the two goal boxes differ from one another. The results derived from studies using such techniques with laboratory rats are equivocal. Studies by McKelvey (1956), Maher and Wickens (1954) and Reynolds (1949) found no relationship between a measure of rate of learning and the magnitude of reward which was measured in such varied ways as feeding time, amount of food provided, and concentration of sucrose. On the other hand, studies by Furchtgott and Salzberg (1959), Hughes (1957), Schrier (1956), Reynolds (1950), Lawson, Cross and Tambe (1959) and Greene (1953) found a relationship between magnitude of reward and learning. These two groups of studies have markedly different outcomes and yet they do not differ one from another in any particular respect. One cannot find any systematic difference in the reward variable or in the nature of the apparatus used. Both groups of studies cover approximately the same range of number of trials. Why the two groups of studies differ in results is hard to say. Consistent with the findings reported earlier by Nissen and Elder (1935) and Cowles and Nissen (1937), investigations in which primates have been used as subjects continue to show a relationship between magnitude of reinforcement and performance in discrimination learning tasks. The studies reviewed, e.g. Meyer and Harlow (1952) and Schrier and Harlow

(1956) are few in number, however, and it may be that more complicated relationships will ensue as more elaborate experiments are conducted.

On the other hand, assuming that the data presently available reflects a real difference in the sort of experimental results that can be expected in the study of primates, one can speculate about the reason for the consistency of results with primates or, conversely, the inconsistency of results with lower species. One factor involved may be that for differences in reward to be effective they must function as discriminated operants. If a rat cannot discriminate between one quantity of food and another, then it can hardly be expected to behave differently in relation to a smaller quantity than in relation to a larger quantity. One may presume that the rat would have very little ability to discriminate quantities of food and would have had little experience in making such discriminations, hence, small differences in size of food rewards would not be expected to exert an influence on behavior except under exceptional circumstances. In few of the studies reviewed did the rats have an opportunity to experience small rewards and large rewards in close temporal proximity. Some experience of this sort is even important for the results obtained with primates (Cowles and Nissen, 1937).

Relationships of Magnitude of Reinforcement to Other Variables

Studies have been undertaken which have related measures of magnitude of reinforcement to other variables. There is almost no limit to the number of other variables to which variation in this condition may be related.

Little has been said up to this point about the relationship of size of reward to extinction. A number of studies provide data indicating that the larger the reinforcement, the greater will be the number of trials needed to reach a given level of extinction. These include studies by Zeaman (1949), Guttman (1953) and Reynolds (1950). This is not to be expected in terms of the results obtained by Crespi (1944) who found that when the level of reinforcement was reduced that the level of performance fell below that which would be ordinarily associated with the lower level of reinforcement. The removal of reinforcements represents the limit of reducing reinforcements and should result in a "depression" effect which might even be expected to amount to almost immediate extinction, but this does not take place.

The partial reinforcement effect originally reported by Humphreys appears to bear a relationship to magnitude of reinforcement in the original training series. Sheffield (1956), Wilson, Weiss and Amsel (1955) and Lewis (1956) found that the partial reinforcement effect occurred mainly with the use of rather large rewards during the acquisition series. Why this is so is quite obscure and not in accordance with any theoretical formulation with which the writers are familiar.

The effect of the magnitude of reinforcement, the strength of drive, and the interactive effect of these two variables on learning has been the subject of two studies by Reynolds and Marx (1952) and Reynolds and Pavlik (1960). In the Reynolds and Marx study albino rats were studied in a bar-pressing situation. The results indicated that with a relatively large reward per reinforcement, high drive animals extinguish more rapidly than low drive animals. In contrast, where a relatively small reward is given per reinforcement, low drive animals extinguish more readily than low drive animals. In contrast, where a relatively small reward is given per reinforcement, low drive animals extinguish more readily than high drive animals. In the Reynolds and Pavlik study the running speed of albino rats in a straight alley maze was found to be a function of both deprivation time for food and magnitude of reinforcement. However, no interaction effect was found between the deprivation time and magnitude of reinforcement which leads to the inference that these two characteristics function in an additive manner. Similar results were also reported by Weiss (1960). In the case of the latter study deprivation time and reward magnitude were studied as variables related to running time of rats in a straight alley maze. The data supported the hypothesis that drive (D) and incentive motivation (K) operate on running speed in an additive relationship.

A final study of interest relates magnitude of reward to alternation behavior. It is a well known fact that when a rat is placed in a two-choice situation such as a T-maze and is then forced to take one choice for which it is rewarded, it will not continue to choose the same alternative when placed in a free-choice situation. Fowler, Blond and Dember (1959) found that under all levels of reinforcement, large or small, rats tended to avoid the forced, reinforced alternative. The latter alternation tendency declined over training and the tendency to select the reinforced alternative was facilitated by using a large reward. The study is particularly important in that it suggests that forcing a behavior and then reinforcing is different in its consequences from the same behavior occurring without coercion and then being reinforced. That magnitude of reinforcement may tend to mask the final difference in learning is also of interest. A similar study or set of studies needs to be undertaken with human subjects.

Relationship of Magnitude of Primary Reinforcement to Magnitude of Secondary Reinforcement

Several studies have been undertaken which purport to relate measures of the magnitude of primary reinforcement to measures of secondary reinforcement generated from them. Such studies generally evade the issue of what is to be considered a primary reinforcer and what is to be considered as secondary. In such studies, the primary reinforcement may be solid food varied in terms of the quantity provided, or in the time of eating permitted, or it may be a sucrose solution varied in terms of the concentration of sucrose. The issue of whether the ingestion of food involves secondary reinforcers is not generally raised by these experimentalists. The secondary reinforcers also showed considerable variation from study to study. A common assumption made is that cues associated with the goal box or the approach to the goal box acquire secondary reinforcing properties. On this basis the generalization of these cues to other goal boxes with characteristics which differ in varying amounts from the original training goal box may be studied.

In the six studies reviewed, those by D'Amato (1955), Powell (1957), Butter and Thomas (1958) and Stebbins (1959) produced positive relationships between the magnitude of the "primary" reinforcement and the magnitude of the secondary reinforcement generated by it. While the primary reinforcement was measured in the ways which have been described, the strength of the secondary reinforcement is generally measured through the response of the organism to an acquisition or extinction situation. The results of these four studies are generally quite striking and added strength is given to their conclusions by the fact that they involve a diversity of primary reinforcers and also a diversity of cues that are considered to function as secondary reinforcers. In all four studies rats were used as experimental subjects.

However, the clear picture of results painted up to this point is befogged by two studies which did not find a similar relationship between the magnitudes of primary and secondary reinforcers. One study by Hopkins (1955) which is closely similar to the study by D'Amato (1955) failed to find any evidence of a relationship between magnitude of whatever is defined as a primary reinforcer and resulting secondary reinforcement effects. The negative results of the Hopkins study may well be due to the limited scope of training involved.

Another study by Lawson (1953) also failed to find the relationship under discussion. In the Lawson study the training of rats occurred in a maze with a white goal box. Cues associated with the white goal box acquired secondary reinforcing properties which could be demonstrated by the fact that rats trained with the white box extinguished the response in which they had been trained more rapidly when a black goal box was substituted than when the original white box was used. However, the strength of the reinforcing properties of the white goal box was unrelated to the strength of the primary reinforcement as measured by the quantity of food provided. Indeed, in one of the two experiments reported, running speed during the extinction series was negatively correlated with the magnitude of the original primary reinforcement. The results of the two experiments reported by Lawson are so contrary to those found by most other scientists investigating similar phenomena that one is inclined to suspect that variables were operating which were not identified. What such variables could be is a matter of speculation, but laboratory animals sometimes have previous histories which, unknown to the experimenter, interfere with studies in which they are used as subjects. Other factors may be involved in the Lawson study but what they are cannot be determined from the report provided and they are probably unknown to the investigator himself.

Some Theoretical Issues Concerning the Operation of Magnitude of Reinforcement

Spence (1956), in presenting his own position on the subject, summarized the theoretical issues involved in the interpretation of studies relating magnitude of reinforcement to aspects of performance in a learning situation. Spence introduced his own position by going back to the position taken by Hull (1943). He pointed out that the most damaging evidence against the position of Hull comes from those studies in which a change in the magnitude of the reward has been introduced after an asymptote has been reached on the learning curve. The change in the reward size was shown originally by Crespi (1944) to lead to an almost immediate moving of the performance curve to a different level, higher if the reward was increased and lower if it was decreased. This finding has been amply substantiated by subsequent studies which may differ with respect to other findings. According to the original conception of Hull with respect to the operation of incentives,a change in the level of incentives, say upward, should result in the learning curve moving slowly upwards to a new asymptote, but the fact is that this does not occur. The transition to the new asymptote is rapid and not at all in accordance with that expected by the Hullian formulation. In addition, the shift in incentive studies also showed consistently the depression effect which also would not be expected in terms of the Hullian formulation.

Even at the time when Hull was formulating his original position for inclusion in Principles of Behavior, Spence favored a motivational interpretation of the empirical findings. Such a motivational approach had been suggested already by Crespi (1942) in what amounts to a phenomenological form who proposed that a change in incentive resulted in a change in "eagerness" of the subject. Spence attempts a much more precise and behavioristic formulation of what might be involved in a motivational interpretation of the phenomenon.

The core concept in Spence's formulation of an interpretation lies in the fractional anticipatory goal response. This conception implies that in a learning situation such as is supplied by a straight alley maze or a T-maze, with a rat as the subject, the goal response R became conditioned to stimulus cues presented by the goal box and by the alley near the entrance to the goal box. Through stimulus generalization, cues in earlier parts of the maze are assumed to elicit fractional parts of the consumatory response which typically occur prior to the presentation of the actual food. These are referred to as r_g which stand for fractional anticipatory goal responses. In addition, these fractional responses r_g become conditioned to introceptive stimulus cues originating in the deeper tissues involved in locomotion and thus become conditioned to the early phases of the instrumental response. According to Spence, the mechanism $r_g - s_g$ has the additional property of raising the level of excitement of the organism according to the vigor with which it takes place and this in turn results in the mechanism having motivational properties.

Spence finds considerable support for the position that the vigor of the consumatory response is related to the vigor of the instrumental approach in the work of Sheffield and Roby (1950), Sheffield, Roby and Campbell (1954), and in the work of Sheffield, Wulff, and Backer (1959), but more recent work by Davis and Keehn (1959) runs contrary to this position. The latter investigation found that the vigor of an instrumental response, or more specifically, the local rate of licking different concentrations of sucrose and saccharin solutions, did not vary, but that the duration of the licking response was related to the reinforcing effect.

Summary

The characteristics of the reinforcer can vary both qualitatively and quantitatively. Qualitative differences, while providing the basis for the organization of the earlier chapters in the review, are not susceptible to experimental investigation (except in comparative studies) unless they can be quantified in terms of some scale, e.g. preference, nutrition, etc.

Quantitative differences also present complex problems, for reinforcers can be quantified in many different ways, e.g. size, weight, number, concentration, etc. Different results have been obtained with different quantifications of the amount of reinforcement.

In spite of the difficulty presented by the problem of quantification, consistent results generally have been obtained in studies in which a simple task has been employed. These consistencies have occurred in the face of variations in the manner of quantifying the reward, in the measure of performance that has been used, and in the type of task employed in the experiment. These investigations have revealed a simple, increasing monotonic function between the measure of performance and the amount of reward.

As with the results obtained in the studies reviewed in the section on drive, the differences in performance are probably more a matter of differences in motivation than differences in habit strength. As rewards are increased there is a decrease in the amount of competing behavior, and the experimental animal attends less to irrelevant stimuli.

Several factors have been introduced to account for the results that have accumulated that are inconsistent with the relationship stated above. It has been suggested that varied experience with the different levels of reward are important, that the experimental animals must have had an opportunity to learn to discriminate the quantities of reward, and that the adaptation level of the animal may be of considerable importance. In respect to adaptation, the reinforcing efficacy of a reward varies as a function of the preceding experience of the animal, relatively stronger responses being elicited when the reward is greater than that previously experienced and relatively weaker responses when the reward is weaker than that previously experienced. This finding, known as the Crespi effect, has received subsequent experimental confirmation. In addition to these factors, the importance of expectation seems to be well established, especially in the case of higher organisms. A monkey that has seen a banana placed under a cup will in all likelihood reject any lettuce that he finds in its place. There is some evidence also that especially small rewards are frustrating; under appropriate circumstances they will elicit a weaker response than a condition of no reinforcement.

In complex learning situations, less consistent results have been obtained; some studies have presented evidence to the effect that performance increases with increases in drive, while others have not. There does not appear to be any consistent differences in the design of the conflicting experiments, but it is possible that some of the differences can be attributed to the principles mentioned above. Certainly the amount of experience the animals have had with varying quantities of reward is inconsistent across the various experiments. This interpretation is butressed by the fact that more consistent results have been obtained in the study of primates, animals that are more adept at discrimination learning.

The magnitude of reinforcement has also been shown to be related to the rate of extinction in simple running responses and bar-pressing situations, the larger quantities producing a response that is more resistent to extinction. Large reinforcers also appear to bear a relationship to the partial reinforcement effect; several studies have shown that this effect occurs mainly when large rewards have been used during the acquisition series.

In studies that have been undertaken to determine the relationship between magnitude of reinforcement and the development of secondary reinforcing effects, most of the results have shown a positive relationship, but two experiments have produced negative results. Indeed, one experiment reported that running speed during an extinction series was negatively related to the amount of primary reward. More experimentation will have to be conducted to explicate the factors that resulted in this finding.

Theoretical accounts of the role of reward magnitude have undergone marked change during the past twenty years. In his early formulations, Hull attributed the effect to differential development of habit strength, but subsequently, he changed his interpretation to one of incentive motivation. His final formulation relied heavily on the motivational function of the fractional anticipatory goal response mechanism and postulated that this factor multiplied with drive in determining excitatory potential. Spence differs from Hull mainly in treating the drive and incentive factors in an additive manner. Recent experimentation has tended to confirm this interpretation. Spence is faced with a little more difficulty in restricting the incentive function to the vigor of the consummatory responses, for, while this may be a factor, there is data presently available to indicate that the duration of the consummatory response may also be an important determinant of subsequent behavior.

Chapter 9

Feedback and Knowledge of Results as Reinforcement Concepts

The reinforcement paradigm and the definition of reinforcement such as that given by Verplanck (1957) have emerged largely in a laboratory setting involving research on subhuman organisms. The extension of the concept to human behavior is relatively recent and largely a result of the writings of Skinner, although the law of effect, which also found its origins in the study of animal behavior, had been freely applied to the description of human behavior.

Skinner (1953) clearly identifies reinforcement in the case of human behavior with events which provide the human organism with information about the consequences of its behavior. In the volume cited, Skinner indicates (p. 78) that approval is a reinforcing event and so too are statements such as "that's right" or "good." These events fall within his definition of reinforcement for they clearly increase the probability of occurrence of the behavioral event on which they are contingent. Later in the same book (p. 96) he indicates that all informative feedback related to the acquisition of motor skills should be classified under the category of reinforcing events. He points out further that informative feedback may be an external or an internal event. A child may be told that he has solved a problem correctly or he may know by checking his solution that it is correct. Thus the concept of reinforcement, perhaps in order to salvage it, is extended to include events which cannot be directly observed but are only inferred. The concept of self-reinforcement has also been introduced to account for behavior which recurs and becomes well established despite the absence of identifiable and observable reinforcing events.

The reason why psychologists such as Skinner have to introduce unobservable reinforcers is that they do not think of the phenomenon as a category of events which, by definition, are correlates of learning in some situations. They think of reinforcement as a universal principle which places them in the position of having to postulate reinforcing events even when none can be directly identified.

The definition of reinforcement given by Verplack (1957) which has been accepted throughout the previous chapters is particularly useful in the study of learning in animals which are highly time bound - that is to say in animals in which the time relationships of events related to learning have to follow in a particular sequence for them to operate. The definition limits reinforcers to events that are contingent upon the occurrence of a particular response. This is a useful limitation in dealing with creatures in whom events significant for learning have to take place within certain circumscribed time limits, and a definition incorporating such time factors circumscribes a useful category of events. The definition presents immediate problems when it is applied to human behavior for the same event which functions as a reinforcer within the scope of the definition may also have precisely the same effect when it precedes the response. For example, Annett reports (1959b) that subjects will show improvement in estimating the number of dots in a pattern when they are told after making a guess how many dots there are or by being told before making a guess. The one event would be classified as a reinforcer in terms of the Verplanck definition, but the same event occurring before the response would not be so classified. The possibility of a reinforcing event being presented before the response is inconceivable in the case of subhuman learners because of the high degree to which they are time bound. The human learner is not time bound to the same degree as lower species and hence a time bound definition of reinforcement is probably of limited value because it excludes conditions which are highly relevant.

Apart from the fact that the human is not time bound to the same degree as other organisms, there are other notable differences in the way in which reinforcements operate. For example, Postman and Sussenrath (1961) have raised the issue of whether the operation of reinforcements is automatic, that is to say independent of the cognitions and perceptions of the learner. Classical studies of the law of effect have generally taken the position that the reinforcing events, described as annoyers and satisfiers, function automatically. Postman and Sussenrath have taken the position that mediating responses must be considered in the action of verbal rewards and punishments. However, they go on to state that the issue of whether the action of verbal rewards and punishments is always or never is completely automatic is not a profitable one. They suggest that what is needed is further exploration of the role played by mediating responses in the action of verbal reinforcements.

If the time restriction is removed from the definition of reinforcement in order to incorporate events related to human learning, then the category of reinforcers becomes so large that it is of doubtful value. On the other hand, to do what is done by so many of the advocates of automated instruction and retain a time bound definition (as if no other events had similar properties) is to set up a misleading category. Such a category implies that the time relationship of the response to the event referred to as a reinforcer is important when the fact is that with human behavior it is often trivial. While events which change response characteristics in human learning, such as guidance, feedback, and knowledge of results, could be classified under a broader definition of reinforcement than the one given, the value of compressing them into a single category is doubtful.[1] A much more useful procedure would appear to be to develop an inventory of such events and then to determine empirically the characteristics which can be used to group them together conveniently. Table 9.1 gives a summary of some of the major events which function in much the same way as reinforcing events except they are not time bound. The table does not attempt to provide a comprehensive statement of events related to human learning, but is designed to bring out the time independence of such events.

In the case of animal behavior there are few events strictly comparable to the events in category 1. The nearest that one can come to such events is the learning of secondary reinforcers. Thus a rat may learn that a light is a signal for the presence of food and such information may be used to facilitate learning in a subsequent learning sequence. Primates may sometimes benefit by demonstration, an event which falls within this category. In the case of human learning there are vastly greater possibilities of providing events of this class through symbolic communications though demonstrations are widely used in the case of motor tasks.

The second class of events occurs during the process of responding or immediately after the response is completed. The events are necessarily informative, but they may also produce affect. Whether the performance of the task itself provides the information or whether it requires an agent or a device extrinsic to the task depends largely on how the task is arranged. A person driving a car may judge his speed, or use a speedometer, or obtain feedback from the highway patrol, or he may use the general traffic speed as a cue. However, driving across the desert in the night, he may be unable to judge speed and have to rely on other sources of information. While in the case of driving there is very clear case for using one source of information rather than another in performing the task, there are many tasks in which the best source of information cannot be so easily determined.

The third category of events has hardly been explored, although it is one widely used in the hope that it will produce learning. The teacher who reviews the previous work of a pupil pointing out trends that need to be corrected is performing in accordance with this category. Military debriefing sessions are also of this character.

Table 9.1

Informative Events Related to Human Learning with Properties Similar to Reinforcers (Including Those with Different Time Relationships to the Responses)

1. *Events occurring before task performance*. These vary from cues to complete demonstrations. The cues may facilitate stimulus discrimination or be response cues. They may provide general ways of programming behavior as in the learning of scientific principles before learning related problems. Some of these pre-task events which facilitate learning are similar to the acquisition of secondary reinforcers in studies of animal learning as when an animal learns to associate a light with the presence of food.

2. *Events occurring during task performance or immediately thereafter*. Feedback intrinsic to the performance of the task produced directly by the performance itself. Knowledge of results indicating degree of goal achievement.

   Either category may be either intermittent or continuous. The information may vary from all that is needed to perform at a given standard to misleading information. Redundant information may also be provided.

3. *Events subsequent to task performance (other than immediate knowledge of results)*

   General critique of performance

   Review of specific points to correct

Information and Cues Before Learning

This is an area which has hardly been explored though every teacher provides information and cues to pupils before they are plunged into a task. The information or cues provided may perform a number of distinct functions: They may provide a complete demonstration of performance of the task and, hence, all of the information needed to produce a complete performance; They may signal when or what to attend to; They may provide sequences of responses to be made coded in various ways; They may provide general rules for performing the task. All of these pre-performance events, if they are successful, program behavior in advance of performance. Such an outcome can occur if the information provided can be retained in a coded form and if the code is such that it can be used as a guide to behavior.

Annett (1959b) studied conditions related to the task of learning to detect an auditory signal near to the threshold. One group was given a flash of light a half second before the auditory signal on trials 2, 4 and 6. A second group was provided with the same flash also on trial 2, 4 and 6 a half second after each signal if the signal had been either identified or missed. A third group was told the total number of correct responses after each series of trials. The group with the advanced information showed the greatest gain on the task and retained it over three five-minute periods. This was better than all other groups at the .001 level of confidence. The phenomenon has to do with perceptual learning which does not fit easily into the reinforcement paradigm.

Annett (1959b) conducted another experiment in perceptual learning involving a rather more complex task. In this second task a display of from 10 to 50 dots was presented tachistoscopically. One group was told the number of dots before presentation while the other group first guessed and then was told the number of dots. Learning was tested on trials during which no information was given. Both groups improved at the same rate, but an experimental group that was provided no information did not improve.

While the information given before performance and the same information given after performance may have identical effects in terms of overall contribution to learning, this does not imply that they involve similar processes. There is the possibility that on Annett's second task, the information given before the task might have facilitated perceptual structuring while that given after might have had a more direct response-strengthening effect.

Another study by von Wright (1957) provides further evidence concerning the effect of information given before a choice point in comparison with information given after the choice-point decision has been made. Von Wright used a paper-and-pencil maze which was arranged so that only a small section could be seen at one time. One group could see far enough ahead so that at each choice point the blind alley could be avoided. A second group had to make a decision at each choice point and subsequently discovered which was the correct alley. A third group followed the winding path to the goal along a track from which all side alleys had been eliminated. The group that was given information prior to the choice point performed better on the criterion trials than the others. As a matter of fact it made less than a third of the errors made by the group provided with information after making each decision.

Several explanations can be given for von Wright's results. One is that when the subject is corrected after making an error he retraces his path, but this is not equivalent to making a correct response at the choice point. On the other hand, the subject given information before reaching a choice point is able to make a correct decision and correct response, both events conducive to learning.

A somewhat similar explanation is provided by Annett (1961) who points out that a subject who makes a correct decision at N choice points would have to store N bits of information, but a subject who made a correct decision and then retraced his steps to make a correct decision would probably store 2N bits of information (of course he would only have to store N bits).

A third area in which studies have been conducted on the effect of information prior to performance goes back to the work of Judd (1908) on the effect of learning the principle of refraction on the task of hitting an underwater target with a dart. While Judd found that the information provided prior to the task did not influence rate of learning on the task as it was initially presented, it did influence performance when the depth of the target was changed and the learner had to modify his procedure. However, when the experiment was reproduced with minor modifications by Hendrickson and Schroeder (1941), a direct effect was found on the initial learning trials - knowledge of the principle did facilitate learning. The learning of such principles has the effect of providing template in terms of which subsequent behavior is guided.

The Judd experiment involving the teaching of an applicable principle prior to the task has been undertaken with corrections. Craig (1953), for example, broke down the guidance to be given in a task involving the identification of similarities into a number of components and found that the more cues that were provided prior to performance the more rapid was the learning and the better was the transfer to a new task. However, the outright giving of the answer to the problem in a way which prevents the learner from undergoing a process commonly described as "discovery" has been shown by Stacey (1949) to hinder transfer.

The evidence indicates that information provided prior to the performance of a task may perform a number of different functions varying from a mere increased sensitivity to stimuli to the internal development of a template for the complete programming of behavior.

While in this chapter we have arbitrarily divided up along a time continuum information carrying events, there are obvious cases in which an event may fall into two categories. There are many tasks in which feedback at the end of a trial may be viewed as strengthening or weakening a response previously made or as contributing to the formation of a template which will guide future behavior.

Guidance During Learning

In the typical instrumental learning situation, conditions are arranged so that the response to be made or closely related responses have high probabilities of occurrence in the subject's repertoire. The occurrence of the response or a closely related response is then reinforced to strengthen it in comparison with other responses. The only event controlled by the experimenter once the subject has entered the learning situation is the contingency of reinforcement. Very rarely is an attempt made to control the behavior of the subject by means other than reinforcement except in the case of the human learner. Perhaps a major reason for the neglect of research into factors other than traditionally defined reinforcers lies in the fact that such events do not fit the theoretical interests of psychologists concerned with the problems of learning. However, there is a group of events which can occur during learning which are generally called guidance which can influence the course of learning. Sometimes these events fit the Verplanck (1957) definition of a reinforcer in that they do influence the acquisition of a response and are contingent on the occurrence of the response, but sometimes they do not. Here again, one is reminded of the fact that with relatively complex tasks with higher organisms, the term reinforcement does not refer to a fairly homogeneous category of events but to events as miscellaneous as the contents of a garbage can.

McGeoch and Irion (1952 p. 282) have summarized some of the classes of events which fall into the category of guidance. The following classification has utilized some of the same categories:

1. Mechanical guidance. This can be undertaken on motor tasks such as that of learning a maze consisting of grooves on a solid piece of wood. The blind alleys can be blocked off so that the learner is confined to the correct channel.

2. Movement control. This would be accomplished in the previous example if the experimenter were to take the learner's hand and move his finger through the maze. Mechanical means can also be provided for making the movements necessary to perform a motor task.

3. Perceptual guidance is illustrated by the study by Gleitman (1955) in which rats were moved through a maze on a trolley. In this case the

movements of the rats were restricted but they were exposed to a stream of inputs of perceptual information.

4. Verbal guidance may be given during learning and may vary in amount from complete directions of how to proceed at each stage (for example, "now multiply that number by that number") to minor cues.

5. Auxilliary feedback devices may be arranged so that the subject will know exactly how to perform. For example, in a plunger pushing task Annett (1959a) provided a visual display which would enable the operator to determine exactly how far to push the plunger for the performance to be correct. The visual information permitted no error.

Such a classification is in terms of the external circumstances involved rather than in terms of the internal events which are controlled.

While in this chapter we are primarily concerned with informative inputs and the emphasis has to be on research with human subjects, some relevant data still have to be derived from animal experimentation.

The problem of providing guidance which will facilitate the learning of a sequence of movements has been studied from two different approaches. The one approach attempts to promote learning by providing sensory inputs. The other involves an attempt to guide movements in some way with the expectation that such guided movements will lay down neural patterns which will in some way guide subsequent performance. The one approach assumes that retention is in the form of a mnemonic schema which is primarily perceptual, the other assumes that retention is primarily a matter of laying down what has been called here an action template which has a more direct effect in guiding subsequent behavior.

Consider the situation involved in moving a rat through a maze on a trolley as Gleitman (1955) did and then determining whether the rat has learned his way through the maze by such a procedure. If memory involves largely the laying down of perceptual memory traces or schema, as Head (1920) proposed, then the rat should benefit from its perceptual experiences. On the other hand, if retention is largely in terms of what has been termed here as template which guides action, then one would expect relatively little benefit from moving the rat through the maze without action on its part. The fact is that Gleitman (1955) found his procedure produced learning as did McNamara, Long and Wike (1956) who moved their rats in a basket. The latter study is of particular interest in that it indicates that the rat could utilize the relevant information only when the irrelevant was cut down.

Another issue pertains to the matter of what is reinforced when learning occurs through such a procedure. The definition of reinforcement from which this monograph took off assumed the position that it is _responses_ that are reinforced. If learning occurs in the absence of the responses that are ordinarily considered to be reinforced then the reinforcement model of learning is placed in jeopardy. Of course, one can always save the model at this point by assuming that there are unobservable responses that are reinforced, but there is danger of ending up with a conception of behavior in which both the response and the reinforcing events are unobservables.

The study of the extent to which learning can occur without the learner initiating a response which is subsequently reinforced has long been a concern of psychologists. Thorndike (1898) was concerned with this problem and attempted to train both dogs and cats to raise a lever to escape from a box by forcibly making them raise the lever with their nose. However, he could find no signs of learning resulting from such a procedure and concluded that learning in such animals required the actual making of a response followed by either reward and punishment. He concluded that the animals were not capable of the ideational processes needed for learning through direct guidance. Later, Thorndike (1901) attempted a similar procedure with monkeys but without any marked success. The difficulty of forcing such an animal to perform a particular act made systematic experimentation almost impossible. There must surely be a great difference between coaxing a cooperative animal to raise a lever with its nose and forcing a reluctant or even rebellious animal to perform an act while its muscles are pulling in the opposite direction. Cole (1907) repeated the experiments of Thorndike but used racoons and found very clear and positive evidence that the majority of his animals would learn the lever raising task through being put through the act. There is a possibility that the very unusual circumstances of the Cole study led to success. Cole had raised a litter of racoons, hand feeding them until they were weaned. The animals became extremely friendly and would follow him on walks through the fields. At the age of a year he began training them in puzzle boxes in which they became very adept at opening doors. Later it became easy to hold an animal and to put it through the task of raising a lever with its nose in order to escape. Cole found it necessary to give the animals as many as 100 forced trials in order to produce a convincing amount of learning and he noted that the forced demonstration was the slow way of teaching.

It is hardly surprising that learning occurs when rats run a maze with the blind alleys shut off as in the case of the study of Carr (1930), for in such a case the rats make the response and are reinforced for it. But the results of Cole are surprising for one presumes that the racoons which had their noses pushed against the lever to open the box would not have the kinesthetic feedback which would ordinarily accompany the instrumental act when the racoons themselves undertook it. Reinforcement theorists, such as Spence (1956), have generally assumed that such feedback plays a vital role in the acquisition of complex behavior requiring a sequence of responses.

Studies similar to those which involve putting an animal through its paces have been undertaken with humans. The typical situation which has been used for this purpose is a stylus maze and guidance is given by gripping the stylus below the part where it is held by the subject. The point is then guided through the correct path. Both Waters (1930) and Carr (1930) conducted studies using that kind of a technique and found that while the subjects made progress towards learning the maze, less progress per trial was made by this means than by allowing the subjects to find their own way and permitting them to make errors. The behavior involved fits far more closely the reinforcement paradigm than does the behavior of the racoon having its nose pushed against a lever. At least the human subject had to make responses not too similar to those which he would have to make if following through the maze without guidance, but he also had the added task of following the movements of the experimenter which might, in turn, disrupt the learning. It should be noted that Carr (1930) also used mechanical guidance with human subjects, involving the blocking of blind alleys, and found that this was of some use provided it did not come very early or very late in learning. His data with humans may well be contrasted with his data from rats which ran the same form of maze. The rats, in contrast to the human subjects, showed very substantial benefits from such guided trials at all stages of learning. The rats, in contrast with the humans, learned more from each guided trial than from a free trial in which they were permitted to make errors. However, Alonzo (1926) who led rats through a maze on a leash found less learning per guided trial than per free trial. An effect similar to that found by Carr (1930) had been found earlier in a study by Kochs (1923) who had provided guidance, not by the experimenter moving the stylus of the subject manually, but by blocking off the blind alleys, Kochs found that rats benefited more by this procedure than did humans. The finding, in the case of the rats, fits well the reinforcement paradigm, but the fact that humans can benefit more from situations in which they make errors than from those in which they cannot is difficult to understand. If the latter conclusion is ultimately found to have any generality for human learning, some progressive designers of learning programs will have to begin to wonder whether the alley in which they have been running really does lead to the anticipated goal of improved learning.

Wang (1925) also studied human learning with a stylus maze task using verbal guidance in two forms. In the verbal instruction form subjects were told step-by-step how to proceed as they moved along. In the verbal information procedure, subjects were told that they had made an error after the error was made. The instruction method proved the superior of the two which suggests, on the surface, that an advantage is gained by arranging for error-free performance. However, the fact remains that only a few such guided trials given early in practice were of value and the rest of the learning had to be undertaken in free trials if it was to be efficient.

An interesting outcome of studies such as those by Waters (1930), Carr (1930) and also that of Ludgate (1924), all of whom studied the effect of manual guidance on the learning of a stylus maze with human subjects, is that the position of the guided trials in relation to the entire sequence of learning trials is important. The general rule which seems to emerge is that in a task, such as the stylus maze, guided trials are most effective if they are placed early in the learning series but not right at the beginning. Guided trials introduced late in learning may actually interfere with what has been learned, - perhaps because they introduce suddenly a series of unfamiliar cues. Guidance performs a much more complicated function than the simple strengthening of a successful response by arranging for the response to occur. The guided trials are more easily interpreted as information providing situations, but the information is easily used only if it is provided at a certain stage of learning. One problem in the use of guidance is that subjects, both animal and human, can become dependent upon the additional cues provided by guidance so that performance without them becomes more difficult rather than easier.

The issue raised by such studies is whether instrumental learning involves the learning of a correct response and the extinction of incorrect responses or whether it involves the learning of a discrimination between cues which lead to goals versus cues which lead to failure. This, of course, is the old controversy of sign learning versus response learning. The concept of response learning leads more readily to experimental paradigms which is perhaps why such a vast amount of data have been collected which fit the concept of response learning.

Augmented Feedback

A special case of guidance as well as a special case of reinforcement exists which have not been considered up to this time. This is augmented feedback. Most of the knowledge available about it comes from research concerned with the training of military personnel. Augmented feedback is a special case of either guidance or knowledge of results in which information other than that normally derived from the task is provided by another source of feedback which is often transmitted through a different sensory channel. Thus a man firing a gun may know when he hits a target, but he will normally have little feedback concerning the size of his error if he misses. However, a training device may be so arranged that a bell rings if the error is beyond permissable limits. The bell provides augmented feedback and, in this case, provides information additional to that normally present. Sometimes, however, augmented feedback provides only information redundant with that already available. Thus in learning to fire a rifle the trainee must acquire the habit of squeezing the trigger rather than pulling it. A training device could be made so that a bell would ring if the learner did not squeeze. Now in this case the bell does not tell the trainee anything which he cannot find out by examining his own behavior. The information is completely redundant with that already available to him. There is also another important difference between the two examples given, namely in one case the augmented feedback is provided after performance while in the other it is concurrent with performance.

Since the initial research on this problem has been conducted for the military, most of the scientific publications occur in sources to which there is no ready access. For this reason, we have had to rely on a summary provided for the U. S. Navy by Annett (1961).

Annett (1961) points out that most of the early work on augmented feedback involved tracking tasks in which performance on the task itself provides intrinsic feedback. Some additional information is provided by an additional feedback loop which augments the information provided by the task itself. Annett cites a study by Biel, Brown and Gottsdanker (1944) in which gunners were assigned the task of tracking a lateral target and in which the instructor was able to follow the performance of the gunner on a check sight. Whenever the gunner pointed his gun outside the target area, the instructor sounded a buzzer. In the particular study cited, improvement was more rapid with the augmented feedback than without it and the gain was not lost after the additional feedback was withdrawn. Annett points out that the augmented feedback can perform many functions in such a situation. First, it can provide information on the tolerances that are acceptable. Second, since the augmented feedback indicates that performance of the gunner is being constantly monitored, incentives are introduced which may influence learning. Many augmented feedback situations do not involve the latter element since the additional feedback loops can be made to function automatically.

Annett (1961) reviews a list of a half dozen or more studies of augmented feedback in tracking tasks. While the earlier studies offered promise that such feedback would provide an effective means of improving learning, the fact seems to be that while a temporary improvement does occur, the performance of groups so changed slips back to the level of control groups once the additional cues are withdrawn. What appears to happen is that the subject becomes dependent upon the augmented feedback cues and, when these are withdrawn slips back to the level of performance expected in their absence. A similar cue dependency has been reported by Alonzo (1926) who studied the effect of leading rats through a maze on a leash. Alonzo reported that some rats became so dependent on the leash that they would not run the maze without it. The best one can say about augmented feedback is that there is no evidence of negative transfer when supplementary source of information is withdrawn.

In order to prevent subjects from becoming highly dependent upon the augmented feedback, Houston (1947) attempted to use a partial reinforcement procedure with the augmented feedback appearing on less than 100 percent of the trials. The groups with varying amounts of supplementary feedback did not differ significantly in their performance after such feedback was withdrawn.

There are some conditions under which the habits of the learner are such that augmented feedback is required for the effective learning of a task. In two learning studies by Twitmyer (1931) and Melcher (1934) subjects learned a stylus maze with and without the use of vision. In both of these studies the maze involved became a much harder task without visual cues. While it is evident that in a maze learning task, vision permits the subject to look ahead and identify blind alleys, the results of the Melcher study are surprising in that most of the children were unable to learn the maze at all without the use of vision. Presumably this reflects a dependency on visual cues.

A particularly interesting set of studies is concerned with the use of augmented feedback in learning a task in which the intrinsic feedback is primarily kinesthetic and probably lacking in precision. In such a case it is interesting to know whether the use of augmented feedback can sharpen the information derived from a sensory modality which ordinarily does not supply information with a high degree of precision. Lincoln (1954a, 1954b) used a crank turning task in which subjects were asked to turn a "handwheel," presumably a crank, at a given number of linear feet per minute. After initial attempts all subjects were told their initial rate error. After their first trial, subjects were informed of their error after each 15-second trial. One group who received visual feedback provided by a light spot which moved with respect to a target. No significant difference was found between those S receiving visual feedback and those that did not. In a second study, following an initial learning period Ss were divided into three groups. One group was given verbal information concerning its error rate after each trial. A second group received kinesthetic information by being asked to grasp the knob of the wheel while it was driven by a motor at the mean error rate of the previous trial. A third group of Ss did not receive any error information, but after grasping the knob of the wheel, the wheel was driven for 15 seconds at the standard rate at which the knob was to be driven by the Ss. The main outcome of this study appears to be that feedback concerning error appears to be more important than information indicating the performance to be emulated. Subjects maintained their level of skill without appreciable deterioration for a period as long as 24 hours, even though the special cues involved in learning had been withdrawn.

A particularly interesting study of the learning of a movement response with varying sources of information is provided by Annett (1959a). His subjects learned to push a plunger by a given amount under varying conditions of feedback. The experiment included conditions in which: (1) subjects could watch a spot of light moving in relation to a target and providing information about the correctness of the position of the plunger; (2) subjects saw a light come on shortly before the target was approached and the light extinguished shortly after the target was passed; (3) the subject was told verbally the reading of his final position with a scale reading of 20 being correct; (4) the spot of light indicating the position of the plunger was covered, but at the point where the plunger was held steady was uncovered so that the subject could see where he was in relation to the target.

While Annett found results similar to those of other studies such as that the removal of an extra cue resulted immediately in a large increase in error, his findings are of interest for another reason.

An important point to note in the design of the study is that under condition (1), and only under that condition, the subject makes the correct response every time for the feedback enables him to correct any error. On the other hand, under the last three conditions feedback is given after a response is completed. The subject then learns something about the error he has made. Now the data showing that learning is least effective under the knowledge of results conditions which does not permit the subject to make an error. The conclusion drawn by Annett (p. 12) is that "The results of these experiments give considerable support for the contention that the perceptual aspects of a task is of overriding importance in learning. To repeat a precise response is not a sufficient condition for learning that response, even if the response is known immediately by the subject to be correct." Annett goes on to suggest that learning of the kind which is undertaken in the plunger pushing task, or in any task in which learning is based on similar feedback loops, is dependent upon the subject learning to discriminate between the feedback associated with error and that associated with a correct response. Under condition (3) there is only limited possibility for such learning. The point is one which is important in connection with the whole matter of the value of error responses in learning. The position taken by the Skinner school that only correct responses should be permitted during acquisition is probably a generalization which applies to only a very limited range of tasks.

Events During Task Performance or Immediately Thereafter

Most tasks performed in educational settings result in some consequent event which informs the subject how he has performed in terms of reaching a goal or achieving a standard. The commonest of such consequent events, in the case of learning undertaken in a recitation setting, are the words "right" or "wrong" or the teacher saying nothing but referring the question to another child. The effects of such events on learning have long been investigated beginning with the numerous experiments of Thorndike discussed in Chapter 5 which led to the controversy concerning the relative effects of "right" as contrasted with "wrong." The controversy which resulted has been reviewed in another chapter.

A long series of researches have been undertaken on the relative effectiveness as reinforcers of various combinations of "right" and "wrong" and no comment. The following combinations are those which are meaningful and typically included in studies:

"Right" for a correct response, "wrong" for an incorrect response (R-W).

"Right" for a correct response, no comment for a wrong response (R-N).

Table 9.2

Research Related to the Use of "Right,"

"Wrong" and No Comment as Reinforcers

	Subjects	Task	Condition	Results
Buss, A.H., Wiener, M., and Buss E. 1954	102 Neuropsychiatric Patients	Block sorting	R - W N - W R - N	R-W and N-W gave a steeper gradient of generalization than R-N.
Buss, A.H. and Buss, E. 1956	45 Neuropsychiatric Patients = Exp. I 55 Neuropsychiatric Patients = Exp. II 24 Student Nurses = Exp. III	Wisconsin card sorting	R - W N - W R - N	No difference between effects of different reinforcing contingencies when shape was the attribute involved. When color was the attribute R-W and N-W equally effective and significantly better than R-N. When number attribute was learned, R-W and N-W were equally effective.
Buss, A.H., Braden, W., Orgel, A. and Buss E. 1956	60, 30, 40 Neuropsychiatric Patients	Block sorting	R - W N - W R - N	R-N leads to slower acquisition than N-W or R-W. N-W leads to faster extinction than the other two. Authors infer from data that R is a weak positive reinforcer and W is a strong negative reinforcer.
Ferguson and Buss, 1959	60 College Students	Block sorting	R - W N - W R - N	R-W and N-W produce more rapid learning than R-N (sig. at .001 level) N-W counter-conditioned less easily.
Buchwald, A.M. 1959a	54 College Students	Pseudo coding nonsense syllables with digits	R - N R - W N - W	Acquisition most rapid with R-W followed by N-W and R-N. On extinction R-N extinguished most rapidly - interprets results as indicating that N becomes negative reinforcer under R-N conditions.

Buchwald, A.M. 1959b	36 Undergraduates = Exp. II 84 Undergraduates = Exp. III	Pseudo coding nonsense syllables with digits	R - W R - N N - W	Exp. II. On acquisition N-W had no fewer errors. Exp. III. A very complex design. Buchwald infers from data that under R-N, N becomes a negative reinforcer, and under N-W, N becomes a positive reinforcer.
Meyer and Seidman, 1960	80 4-5 year olds 80 8-9 year olds	Concept learning task-learning "larger than" relationship	R - W R - N N - W Buzzer-N	On acquisition the reinforcer effectiveness was in the following order for the preschoolers: R-W, R-N, N-W, B-N. For the older children, R-W, R-N, N-W, B-N. These were manifested by significant (.001) interactions in an analysis of variance on extinction both groups extinguished least rapidly on R-W.

No comment for a correct response, "wrong" for a wrong response (0-W).

The studies which have been undertaken comparing the effectiveness of these various combinations of reinforcing events are summarized in Table 9.2. A first point to note in the examination of the Table is that a majority of the studies have been undertaken with neuropsychiatric patients, though some have been undertaken with other populations. Second, the outcomes of the experiments are surprisingly uniform; the combinations R-W and 0-W are close to one another in their effectiveness and superior to the combination of R-0. Meyer and Seidman (1960) found an age effect suggesting that the younger group of kindergarten subjects had more difficulty in responding to 0 as a reinforcer than did the older group. Perhaps it takes a certain degree of maturity to respond to 0 as a reinforcer.

How 0 comes to be a reinforcer is a matter of controversy. Buchwald (1959a, 1959b) suggests that when it is paired with R it becomes a negative reinforcer, while when it is paired with W it comes to have positive effects. The studies do not throw much light on the controversy from which they arose, namely, the relative strength of R and W. While it seems clear that 0-W is a more powerful reinforcer than R-0 this difference could be attributed to reasons other than differences in the power of R and W. It could be that 0 acquires different power when combined with R than when combined with W.

The results of these studies are of particular interest in view of the fact that the most commonly observed pattern of reinforcement in school recitation situations is that of R-0, which turns out to be the poorest combination.

Effect of Variation in the Information Transmitted by a Reinforcing Event

While one important difference between the application of reinforcement concepts to subhuman and human behavior lies in the fact that man is not time bound to the extent that are simpler organisms, another important difference lies in the quantity of information that can be transmitted. One can conceive of one aspect of reinforcement as an information transmitting system in which the message transmitted results in the programming of behavior so that a particular end state is achieved - the end state being either a drive reduction condition, a situation involving a high rate of responding, a situation involving a particular sensory input, the description depending upon the habits of the research worker. Such inputs of information resulting in the programming of behavior are presumed to be very small in the case of an animal such as a rat, but also probably limited in the case of the human learner. One cannot estimate the information carried by a single reinforcement administered in a typical animal experiment, but one can arrange learning situations with human subjects so that the amount of information per reinforcement is known. This can be done where the experimenter has clearly in mind just what he wants to reinforce, but not in cases where the reinforcement is very imprecise and the experimenter is not sure what exactly he is reinforcing. Knowledge of results in typical concept formation tasks represents reinforcement involving very precise information given for a precise purpose. In contrast, saying "good" as in the Maltzman, Bogartz, and Breger (1958) study of reinforcing creative behavior represent very vague information, - only telling the subject that the word given either belongs in the desired class, or that the relation of the word given to the stimulus word falls in the class of desired words, or that the length of the word is appropriate, that this particular word should be used, and soforth. Reinforcements of the latter class, even when the subject utilizes all the information provided, have very limited capacity for programming behavior.

In contrast, information given in concept learning tasks provides precise information which can be used in the programming of behavior. Again, in many motor tasks the knowledge of results provided either during or after performance may have the potential of programming behavior precisely to reach some end state. This is a major difference of the function of many reinforcing events applied to humans in contrast to those in the training of animals. This distinction is very similar to that made by Elwell and Grindley (1938) between the straightforward operation of the law of effect and events which also supply directional information.

Research on the extent to which learning is improved by increasing the amount of information provided by knowledge of results appears, on the surface, to provide contradictory results, - but the apparent contradictions can be resolved. In a repetition of the famous Throndike line-drawing experiment using blindfolded subjects, Trowbridge and Cason (1932) provided knowledge of results of varying degrees of specificity. The data indicated that the more specific the feedback the better was the learning. The highest degree of specificity of the feedback, in this case, was the reporting of the length of the line drawn. Waters (1933) studying the estimation of a 12-second time interval found a similar relationship between learning and the amount of information given. On the other hand, when the task involved the judging of the length of cardboard strips no such relationship was found. In an old experiment by Brown (1910) involving the judgment of differences between weights, accuracy of knowledge of results had little to do with the acquisition of skill.

Annett (1961) has reviewed a number of studies conducted by the military in which the specificity of knowledge of results has been a variable. These involved mainly simple tasks such as pulling a lever a given amount or turning a knob through a given angle. The overall view of these studies presents a picture showing an extraordinary lack of relationship between the precision of the information provided after each trial and the level of skill acquired.

A single study of the effect of variations in feedback on the performance of a complicated problem task is described by Donahoe (1960). The task involved a game-like arrangement described to the subjects as one involving the hitting of targets on an ocean map. Knowledge of results was either given by reporting the coordinates of attempted hits, or in a compressed form which just gave the overall error. Subjects performed best when the compressecd information rather than the full information was provided.

The data imply that along the path of data reception, storage, and utilization, considerable compression takes place. A subject may well not differentiate in his behavior the differences between the statement "Your error was 6 millimeters" and "Your error was 10 millimeters." Both messages may well be compressed so that the outcome is a small adjustment on the next trial. The problem appears to be not in the intake of information but in its utilization. In most of the tasks involved in the studies reviewed by Annett, the nervous system certainly has the capacity to receive and store the information provided as feedback. The limiting factor is converting it into action.

Feedback indicating the overall level of performance appears to have very little effect on subsequent performance when the task is fairly long and complicated. For example, Crafts and Gilbert (1935) used a maze-learning task with college students. One group was given a score for overall performance at the end of each trial as well as information on a standard of performance to be expected. The other group learned the maze without such information. No difference between the two groups was found either on the original learning of the maze or on a measure of retention applied a week later. A somewhat similar result was found by Ross (1933) whose data showed that knowledge of performance on tests given during a college course had no effect on achievement in a final examination. Both of these studies were based on the hypothesis that knowledge of results has a motivational effect and that the information provided in the particular instances would not, as such, affect performance.

Delay in Knowledge of Results

A review was made in Chapter 7 of the conditions under which reinforcement can retain its effectiveness even though delayed in sub-human subjects. In such studies it has been shown that a major factor which determines whether delay will or will not interfere with the operation of reinforcement is the degree to which the animal maintains an orientation towards the goal. Such an hypothesis would be much more difficult to test in the case of human subjects in view of the fact that task orientation is much less dependent upon observable conditions of the musculature. The difficulty of controlling task orientation in human subjects is brought out in a study of Greenspoon and Foreman (1956) using the familiar Thorndike line drawing task with the eyes blindfolded. The subjects were asked to draw a 3-inch line and to maintain the pencil at the end of the line until told how accurately they had performed. Delay in knowledge of results up to 30 seconds was related to reduction in learning. However, in a final note Greenspoon and Foreman note that no such relationship with delay had been found on a previous experiment which differed only in the fact that the subject had returned his pencil to the starting point immediately after completing the line.

Effective reinforcement after delay would require (1) that the organism retain information about the characteristics of the response that has been made and that (2) this information be subject to some kind of correction process through the information contained in the reinforcing event. If the information under (1) is retained in some kind of short-term storage such as Broadbent proposes (1958) then one would expect the reinforcing event to have to occur within a few seconds after the response. In the case of the Greenspoon and Foreman experiment, by maintaining the pencil point in its final position, the proprioceptive inputs which occur on the completion of the response were maintained long enough so that the information they provided about the response could be modified by the informational reinforcement. On the other hand movement back to the initial position would destroy such feedback and the reinforcing event would not have an existing informational structure to modify. One may further hypothesize that the modified information about a response would become assimilated with a memory trace or what we called here a template for controlling action.

The position that knowledge of results will be effective if it permits the stored information concerning the response and the information from the external source to be collated is consistent with the results of a study by Lorge and Thorndike (1935). The task in this study involved throwing a ball over the shoulder to hit a target. Delay in knowledge of results from 0 to 6 seconds had little effect on performance, but after introducing an additional throw before information was given, no gain was derived from the feedback. The additional throw may well have disrupted the retention of information about the throw concerning which knowledge of results was given. Without the retention of such information knowledge of results could have little effect.

Bilodeau (1956) using a different task, a lever positioning task, did not find that delaying knowledge of results until one or more additional trials had taken place obliterated its effect. However, the effect was reduced increasingly as feedback was delayed from 0 to 5 trials. These results are not necessarily inconsistent with those of Lorge and Thorndike. In lever positioning, feedback may slowly modify an incorrect trend in behavior, such as the tendency to push the lever too far. Thus a subject may have a concept of how far the lever should be pushed. Information may modify this concept rather than the information retained from a particular response. Throwing a ball over the shoulder is probably a much more erratic performance, and what happens on one trial may have little relationship of what happens on the next, and hence knowledge of results has to be given after each trial to be effective. On the other hand lever positioning may be in terms of a fairly stable action template which undergoes only slow modification.

Archer and Namikas (1958) found no effect of delay in feedback, but nobody can be surprised at this. Subjects performed on a rotary pursuit task which provides excellent intrinsic information concerning performance. In addition, a 1000-cps tone was heard after being on a target continuously for periods of 0.0, 0.2, 0.4, 0.8 and 1.6 seconds. Different delays of information feedback had no effect on performance, perhaps because subjects were already well informed by the apparatus itself about how they were doing. However, a similar experiment previously conducted by Reynolds and Adams (1953) had found that a click after 0.5 seconds on target had produced the best performance.

Motivational Properties of Reinforcing Events

While reinforcing events have complex information-transmitting properties, there is considerable agreement that they also have motivational or activity arousing properties. Here again one is reminded of the strong case made by Premack (1959) for the position that reinforcing stimuli are those which arouse a high rate of responding. Certainly, a stimulus to which there was no response could not be a reinforcing condition. The arousal of activity is an important component of a reinforcing event. Some psychologists would take the position that the motivational component is the main component. Mowrer (1960a), for example, makes the outright statement that the problem of training a rat in a Skinner box is to make the rat want to press the lever, for any healthy rat has the capability of this action. Mowrer links reinforcement to motivation through a complex system of constructs postulating that what he calls either "anxiety" or "hope," which are motives, as playing a role in relation to all reinforcing situations. Young (1959) has held consistently that reinforcement in typical animal learning situation involves mainly a hedonistic component.

There is ample evidence to support the view that reinforcements have components other than informational or drive reducing. Such evidence include the work of Sheffield and Roby (1950) and the series of related studies. The work on avoidance learning has shown a proliferation of researches in which the informational properties of a reinforcer have remained the same, but some of its other characteristics have been varied. Our problem here is to consider how events generally classified as knowledge of results may have properties other that the transmission of information and the extent to which such properties may influence the course of human learning.

A typical approach to this problem has been that of comparing learning in which knowledge of results is given by a click with learning in the same situation and with the provision of the same information provided by a powerful stimulus such as an electric shock which is believed to have important properties in addition to that of providing information. Similar research with animals, which present difficulties of controlling the amount of information transmitted by a stimulus, has been reviewed in an earlier chapter. The results generally indicate striking differences in learning when aversive properties are introduced.

Research with human subjects shows much less striking differences between maze learning in which the passage is blocked in wrong alleys and with a learning condition in which the same information is transmitted with a rather strong stimulus. Gilbert and Crafts (1934) who studied this problem with adult subjects found only insignificant differences between a group informed of an error with a click and a group informed of errors with electric shock. However, different results were obtained when Bernard and Gilbert (1941) used a stylus maze and compared performance when blocked from going down blind alleys with performance when shocked for entering blind alleys. All subjects were both blocked and shocked but on different trials. Errors resulting from shock were more quickly eliminated than those which were blocked. They interpreted their results in terms of the hypothesis which has emerged from animal studies, namely, that electric shock produced a hightened sensitivity to cues.

Studies of the effect of failure and the transfer of such effects to other tasks have already been reviewed.

Studies in which a reward has been added to information received are hard to find. Such studies are, inevitably, cues in which some subjects, on new trials, are provided with knowledge of results alone, but others introduced incentives. The latter involve the receiving of some highly valued object such as money or the occurrence of some highly valued state of affairs such as approval.

A study by Bahrick, Fitts, and Rankin (1954) throws some light on the effect of adding reward to knowledge of results. In the latter study, subjects were assigned to a tracking task which involved the adjustment of a follower over a target. The task itself provided knowledge of results for the subject could see when he was off target. Two incentive conditions were provided by telling Ss either that the trials were just practice trials or that a bonus ranging from 5 cents to 3 dollars was to be given for good performance. Intermittent peripheral stimuli were also provided under three conditions differing in the amount of relevance to the reward involved for good performance and in the ease with which they could be detected.

The results of the study showed that the high incentive to perform produced an effect mainly by increasing performance on the central task, to the detriment of performance in the peripheral task. The various experimental conditions appeared to affect behavior on the peripheral task much more than on the central task, where the effect was relatively slight.

The research cited supports the position that the addition of reward to knowledge of results produces greater selectivity to the stimuli to which attention is directed. A considerably smaller effect is shown on the performance of the central task.

One special case of adding reward or punishment to the informative aspect of reinforcement is that in which knowledge of results of performance are correlated with the magnitude of the reward provided. This condition is referred to by Logan (1960) as correlated reinforcement. In human learning studies it would be represented by those in which the size of momentary reward is related to the effectiveness of performance. The magnitude of the reward thus becomes the transmitter of information. Just how successfully such a drive can be for the transmittal of information will depend upon the learner's ability to discriminate one level of reward from another. Research on this problem in such a context as school marks and grades, where it could be a matter of considerable importance, needs to be undertaken.

Effect of Practice Without Reinforcement

For those who regard reinforcement as a general principle of learning rather than as an empirically defined class of events, the postulation of hidden reinforcers to account for learning in situations, where observable reinforcing events cannot be established, has become a challenge to their ingenuity. On the other hand, those who doubt that reinforcement can be considered a universal principle of learning have exercised equal ingenuity in attempting to identify situations in which even the most imaginative reinforcement psychologist has difficulty in postulating unobservable but real reinforcing events. A detailed review of the literature pertaining to this contest of wits would require another volume if it were to be covered exhaustively and if all the blind alleys which the controversy had covered were also to be explored. All that can be done here is to point out briefly some of the studies which indicate that a real issue is involved and to indicate that those who explore reinforcement phenomena would do well to consider them as events belonging to a defined class rather than as exemplars of a universal principle.

The controversy concerning knowledge of results as an essential factor in human learning began, as all readers know, with the classic experiment of Thorndike on line drawing (1931). In the Thorndike experiment subjects were required to draw a 4-inch line from 175-200 times a day for 12 days. Thorndike could find nothing in the successive performance to suggest learning and implied that performance at the end of the series was roughly the same as performance at the beginning. When the data of the Thorndike experiment were reexamined by Seashore and Bavelas (1941) the conclusion was drawn that changes did take place in the performance, - subjects tended to establish a particular length as representing the task requirement and then reducing their variability about that value. The learning which occurs under such circumstances (if one wishes to define learning to include such phenomena) is similar to that which psychologists in the area of perception have described as perceptual learning. A review of a decade or more ago by Gibson (1953) shows clearly that in a vast range of perceptual tasks, subjects improved simply through being exposed repeatedly to the stimulus situation. Improvement takes place through practice, but without knowledge of results, in the case of numerous different functions which include both peripheral and foveal visual acuity, the two-point threshold for touch, and weight discrimination tasks. Differential thresholds also improve, almost regardless of the sensory modality involved.

While Eleanor Gibson points out that in the case of a two-point threshold one can concede that the task presents clear cases of "oneness" and "twoness" and hence the subject can check himself and learn through a reinforcement mechanism, not all of the findings can be accounted for in terms of a concept of "self-reinforcement." She cites the case of improvement in the comparison of lifted weights as one in which one can hardly conceive of self-reinforcement as operating. Another case is that of the tachistoscopic viewing of letters under dim illumination which resulted in improved accuracy of identification. In neither case can one readily conceive how either intrinsic knowledge of results or reinforcement could operate.

More recent studies have been summarized by Solley and Murphy (1960). These also generally point in the same direction. Such improvement as occurs is limited to only certain tasks in which the development of perceptual sensitivity is the core of what is to be learned, but even with such tasks negative results have sometimes been achieved. For example, Minturn and Reese (1951), in a task involving the estimation of the number of dots presented tachistoscopically, could find no effect of practice without knowledge of results. Where knowledge of results is provided in addition to practice most, but not all, perceptual learning is vastly improved.

A learning-by-practice effect has not been generally demonstrated in the case of motor skills or instrumental learning. It is unnecessary here to review the long history of experimentation with animals related to the latent learning issue, beginning with the work of Blodgett (1929) and still continuing. In the interpretation of such research one can never know whether hidden secondary reinforcers are, or are not, operating. The Blodgett type of design, even with all the refinements that have been introduced into it, does not seem capable of providing a crucial test of the learning-without-reinforcement hypothesis.

The more recent studies undertaken by Postman and his associates have been designed to throw light on the conditions that are related to incidental learning. These studies empirically relate what has been termed incidental learning to certain well-identified learning conditions, but they contribute little to the debate whether a pure perceptual learning phenomena exists outside of the reinforcement paradigm. Perhaps one of the most interesting findings which come out of this empirical approach is the finding of Bahrick (1954, 1957) that high motivation related to a task results in less incidental learning than low motivation. This finding fits well with the research indicating that one function of motivation in learning is to restrict the range of stimuli to which the animal will respond.

A number of studies have been undertaken with human subjects involving motor tasks in which knowledge of results has been withheld during some trials. Whether learning does or does not occur on these trials appears to depend largely on the nature of the task. In a lever positioning task studied by Bilodeau, Bilodeau and Shumsky (1959) there are few opportunities available for perceptual learning to occur and hence it is hardly surprising that no learning occurred on the trial when knowledge of results was withheld. The task required the subjects to move a lever by a given amount. On the knowledge of results trials the subjects were told the magnitude and duration of their errors. A similar result was found by Garvey and Knowles (1954) using a task involving the pointing of a joystick in the direction of a target without knowledge of results. Here again there would appear to be few opportunities. The search for situations in which learning occurs without reinforcement may be as rewarding as the search for reinforcing conditions.

Summary

The present chapter has been mainly concerned with experimental results which have been obtained when the temporal factors have been modified in such a manner that the learning situation no longer fits well the reinforcement paradigm.

In the study of human learning, there is a very strong tendency to apply the principles of learning, unmodified, as they have been derived from experimental investigations with subhuman species. Concommitant with this is a tendency to assume that if learning is to occur it is because the response is reinforced. Within the conditions prescribed by a definition such as that tendered by Verplanck, a reinforcer necessarily follows the response which is to be reinforced, for the response contingency is a necessary aspect of the definition. This definition, which has been of startling utility in the laboratory, fails to encompass all of human learning, for humans will, for instance, show learning when the "reinforcing event" precedes the response. Humans are not time bound to the extent as are laboratory animals. The results of this have been manifold. One of the more interesting features is that psychologists such as Skinner have found it necessary to introduce the notion of self-reinforcement or, more generally, the class of unobservable reinforcers. This procedure is perfectly acceptable in those situations in which experimental control is maintained over the conditions under which unobserved reinforcement will occur; but if, as is frequently the case, reinforcement is inferred simply in order to maintain the supremacy of reinforcement as a universal principle of learning, then the principle is no longer susceptible to experimental invalidation. Further difficulty is encountered in the situation in which mediating responses are also postulated, for this leaves the theorist in the peculiar position of asserting that it was an unobserved event that reinforced the response he wasn't able to record.

Since the alternative of removing the temporal restriction from the definition of reinforcement is not an attractive one, the remaining events that are correlated with a modification of response characteristics must be examined on an empirical level with the hope that appropriate theoretical interpretations will eventually emerge from the investigation. For present purposes, these events are reviewed under the headings of guidance, feedback, and knowledge of results.

Guidance refers to all information, cues, and constraints intended to direct the response. These may occur either before or, in the case of a task that requires some time to complete, during the performance of the response. What little work that has been done in the area indicates that the provision of information prior to the presentation of the task may perform a variety of functions. These include the development of increased sensitivity to particular stimuli, perceptual structuring, and the formation of templates for the complete programming of behavior.

Guidance during learning constitutes a particularly broad category which includes mechanical guidance, perceptual guidance, verbal directions during the course of the task, and auxilliary feedback. Early experimentation in the area of mechanical guidance, in the manner of forcing the animal to make the appropriate response, did not produce encouraging results and there has not been sufficient experimentation conducted in more recent years to necessitate a change in interpretation. What learning that has been demonstrated has been particularly inefficient, but if guidance is provided by restricting the possibility of wrong responses (e.g. by blocking the blind alleys in a maze) there may be some facilitation of learning. Rats seem to profit from this experience at all stages of learning, but humans, to the extent that they have been studied in this sort of arrangement, seem to profit only if the guided trials take place early but not right at the beginning of the series of trials. Guided trials introduced late in learning may actually interfere with what has been learned.

Perceptual guidance has not been investigated to any great extent. Two studies involved a procedure whereby the animals (rats in both cases) were moved through the maze either on a trolley or in a basket in order to provide the animals with an opportunity to learn the layout of the maze. Actual performance of the running response was not permitted. In both cases, the procedure produced some learning and the data indicated that the prior exposure to the maze facilitated the subsequent learning of the maze.

The results obtained in experiments conducted with a procedure of verbal guidance fit those of mechanical guidance. The benefits derived are generally slight and are greatest if the guided trials come early in the learning series.

Studies in auxillary feedback have been conducted mainly in applied training situations, the majority of these having been conducted under the auspices of the military services. This technique involves the provision of an additional source of feedback that is not intrinsic to the performance of the task. The majority of these studies have also indicated that the provision of additional cues result in a reliance on the cues with the consequence that the withdrawal of the cues has a deleterious effect on performance. The exception to this principle comes in the situation in which the cues provide information concerning the tolerance limits imposed on the task. Better performance may accrue from the additional incentive the subject has when he knows that his performance is being monitored. A further finding that is of considerable significance is that it is not sufficient to provide for the correct response (as is done when auxillary feedback gives the subject continuous information concerning the adequacy of his response and he continues to respond until the response is correct), for it is necessary for the subject to learn to discriminate between the (kinesthetic) feedback associated with error from that associated with a correct response.

Knowledge of results may be given immediately after the response, or after some period of delay. It may further be varied in terms of the amount of information communicated to the subject, may be categorical (right or wrong), and may consist of a precise statement of the amount of error in the response. The results of experimentation on the relative efficacy of "right" and "wrong" for reinforcing student behavior are presently sufficient to indicate that the method of reinforcing correct responses and ignoring incorrect responses is, at least in the sort of situations investigated, the least effective method of shaping behavior when there is some particular knowledge that is to be transmitted.

Several studies have been conducted on the problem of the role of knowledge of results in the development of psychomotor skills. If knowledge of results is withheld in this type of task, the subject tends to reduce the variability of the response on successive trials. In experiments in which some knowledge of results has been given, it has often but not always been shown that the more specific the information, the better the development of the skill. The exceptions to this principle seem to stem from two major factors. The first is that compressed information is sometimes better suited to discriminable aspects of the response. The second factor pertains to the amount of information concerning the response itself that is retained by the subject. In a line drawing task, for instance, the subject will benefit from information if he is permitted to hold the pencil at the end of the line until information concerning the performance is given, for he has still available the proprioceptive stimuli associated with the extent of the line. But if he returns the pencil before information is given, the knowledge, no matter how specific, will be of little benefit. This reveals to some extent why an overall evaluation of performance in a complex task is of little benefit on future occasions. The subject, again, retains little information about the specific responses that have led to the performance.

In addition to the information they transmit, reinforcing events have motivational properties. Several studies have been conducted for the purpose of comparing the performance of subjects that are simply informed of the correctness of a response with the performance of subjects that in addition are given some sort of reward or punishment. Differences between these conditions almost inevitably occur. The effect, on improvement in performance, seems to be attributable to an increased sensitivity to the relevant stimuli. This is true both in those studies using shock and those that have some added reward. The increased attention to the central task is obtained at the expense of attention to both peripheral stimuli and to peripheral task performance (should the design employ such tasks).

The question of the effect of practice without reinforcement is a touchy problem. If one strictly adheres to a positivistic definition of reinforcement, there are innumerable demonstrations of learning without reinforcement in the literature, for this definition defines reinforcement as the experimental presentation of a reinforcer. This implies that the reinforcer is under experimental control, a condition that frequently is not met. On the other hand, any time a response is in any sense instrumental, then this implies that the behavior is motivated and Skinnerian theorists explain that the consequences of the act are reinforcing. Mainly because of this sort of rationale, the literature bearing on the question of the effect of practice without reinforcement has been accumulated in the area of perceptual learning. In this area, many studies have been reported that at least necessitate the concept of self-reinforcement, and studies that show an increase in perceptual proficiency, such as is demonstrated in the comparison of weights, constitutes a challange for the most ardent reinforcement theorist.

The most recent trend is to sidestep the issue of the importance of reinforcement and to pursue empirical investigations concerned with the parameters that affect learning that is not deliberately reinforced. This type of incidental learning, for instance, has been shown to vary inversely with the drive level of the animal.

Footnote

[1] The following definitions are provided:

Knowledge of results is defined as information received concerning the goal achievement value of a particular response. It may indicate the degree of goal achievement or the standard to be met or both. Knowledge of results may be either intrinsic and a product of the performance itself, or extrinsic and involve coded information which does not constitute a part of the performance.

Feedback is a particular case of knowledge of results in which information is provided which is directly related to some characteristic of the response. Feedback is generally continuous rather than intermittent as are most other cases of knowledge of results.

Guidance consists of any set of cues which indicate how the learner is to perform and which are provided before or during performance. Such cues may either sensitize the learner to particular stimulus aspects of the task or may indicate directly what response to make or how to make it.

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